Mathematics and the world
Explanation and representation

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This thesis is dedicated to the memory of Jim McConville

Abstract

This thesis is about two ways in which we use mathematics to understand the non-mathematical world: in particular, mathematical explanation and mathematical representation. In chapters 1 and 2, I motivate the project by suggesting that, in addition to shedding light on the nature of explanation and representation, it is necessary to develop accounts of these two world-oriented uses of mathematics in order to evaluate competing considerations in favour of, and against, mathematical realism. In chapters 3 and 4 I discuss extra-mathematical explanation. In chapter 3, I consider and reject four recent accounts of mathematical explanation. In chapter 4 I discuss and endorse what I call the modal account of extra-mathematical explanation. I argue, in line with Jansson and Saatsi and contra Baron, Colyvan and Ripley that such an account does not require countenancing counterpossibles, I discuss *in virtue* of what a mathematical fact can play this role and I address whether or not extra-mathematical explanations are causal. In chapters 5 and 6 I discuss mathematical representation. In chapter 5 I consider two fundamental challenges to developing an account of mathematical scientific representation: the first is Callender and Cohen's claim that there are no special problems of scientific representation and the second is a set of influential objections owing to Frigg and Suárez that take aim at accounts of representation that appeal to the notion of structural similarity. In chapter 6 I argue that two recent accounts of mathematical representation are, in fact, complementary and, more generally, that mathematical representation is a special kind of epistemic representation. I draw on some work from epistemology to address, and argue against, Pincock's claim that in order to *understand* a mathematical representation one must believe its mathematical content.

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Contents

Introduction

This thesis is concerned with shedding light on two ways in which we use mathematics to navigate the world: our uses of mathematics to explain and to represent. The project has twin motivations. The first concerns a relatively recent turn in the debate over the ontology of mathematics. Recent debates about whether or not we have good reason for believing in the existence of mathematical objects have taken as their starting point the uses that we put mathematics to in understanding the non-mathematical realm. Just as it is necessary to invoke atoms and bosons in order to organise sense experience, this Quinean thought goes, so must we invoke mathematical objects. The assessment of these arguments has turned recently away from the mere fact that mathematics *is* used in our scientific theories and towards increasingly delicate questions about *how* mathematics is used. As a consequence, mathematical explanation and mathematical representation now play a crucial role in these debates. The second motivation concerns questions about explanation and representation themselves, quite apart from ontological concerns: questions about whether or not there is something core that makes all explanations explanatory and all representations representational and how mathematical explanation and mathematical representation relate to explanation and representation *simpliciter*.

0.1. The metaphysical commitments of world-oriented mathematics

According to mathematical realism, or Platonism, mathematical objects exist.[1] According to one recently popular line of thinking, mathematics plays an *explanatory* role: furthermore, this special kind of role is connected in the right way to ontological commitment such that mathematics playing an explanatory role constitutes a reason to endorse mathematical realism. According to nominalism about mathematics, mathematical objects do not exist. A line of thinking pulling away from realism and towards nominalism says that mathematics only plays a role in *representing* facts about the world: mathematics does not play a role in explaining aspects of reality – rather, it is merely a language in which we can speak about objects that exist.

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[1] This form of realism should be distinguished from a weaker variant, which I will call truth-value realism. According to truth-value realism, there are mathematical truths. Whether or not one can consistently assent to the truth of mathematical statements whilst remaining agnostic regarding the existence, and nature, of mathematical objects standardly taken to be their truth-makers is a fraught question that I will occasionally turn to. Another way of drawing up the realist/non-realist divide, along the lines of objectivity, will be discussed briefly in §1.1.

It is a much improved state of affairs that the debate has moved from the discussion of the fact that our scientific theories contain terms that (purport) to refer to mathematical objects to a discussion of the different roles that mathematics can play in our scientific theories. Yet, too often, disagreements about the kind of role mathematics is playing in this or that case seem to amount to a butting of intuitions: it is unclear exactly what the criteria are for demonstrating that mathematics is playing a certain role in a certain case and equally unclear what our reasons are for thinking that the role being played is one that is linked up in the right way to ontological considerations. This thesis takes as its jumping off point the very natural thought that both of these thorny questions can be better navigated when equipped with *existing* accounts of scientific representation and explanation. Even though some recent contributions to the literature still make do without appealing to any developed account of these phenomena and appeal only to general, symptomatic, features of explanations such as their generality (e.g. Baker 2017), the literature has in general also quite recently taken a similar turn towards developing accounts of these phenomena.[2] This is a positive development, one that this thesis contributes to by adjudicating some of the disagreements that have emerged and by putting forward some positive proposals that compare favourably to what has come before.

0.2. Easy and natural mathematical metaphysics

It is worth briefly mentioning, and then putting to one side, some other considerations offered in favour of mathematical realism. Schaffer offers the following argument for realism:

> Here, without further ado, is a proof of the existence of numbers:
>
> 1. There are prime numbers.
> 2. Therefore there are numbers. (Shaffer 2009: 357)

According to *easy arguments* for mathematical realism, our assent to the truth of claims like '3, 5 and 7 are prime numbers' secures realism near-trivially. A natural thought, however, is that the plausibility of such arguments is likely to rise and fall with the plausibility of easy arguments in ontology *generally* and on the good standing of the recent move in analytic

[2] Only some such accounts are developed with ontological considerations in mind.

metaphysics away from discussion of existence towards questions of fundamentality.[3] Opponents of easy arguments argue that there is no entitlement to reading sentences like 'there are prime numbers' at face-value, and that doing so borders on begging the question against nominalist opponents (Contessa 2016), whilst proponents of the arguments suggest that it is the nominalist opponents who beg the question by suggesting a reading of such sentences that does not involve their truth (Thomasson 2013). There are also responses to such arguments that do not involve matters of dialectical burden and question-begging: for example, it should perhaps be treated as an open question as to whether or not the truth of such sentences does indeed entail the existence of mathematical objects (for two contrasting strategies for answering it negatively see Azzouni 2004 and Nutting 2017).

Related to easy arguments are arguments that appeal to a distinctive kind of naturalism about mathematics. On one plausible line of thinking, the sorts of reasons that speak in favour of believing a mathematical claim are those offered in mathematics. This form of mathematical naturalism is most prominently discussed and endorsed by Penelope Maddy (Maddy 1997; Maddy 2007; Maddy 2011). There are distinct views about what this sort of mathematical naturalism entails: in the most recent iteration of her views on these matters, Maddy maintains that descriptive facts about mathematical practice are neutral between Arealism (roughly nominalism) and Thin Realism, the view that all there is to say about objects like sets and numbers is what set theory and number theory have to say about them (Maddy 2011: 103-112). On this sort of line, only facts about mathematical practice (or, perhaps, only the facts *discovered in* mathematical practice) are the right kind of facts to settle questions about realism and nominalism, but these facts yield an indeterminate answer. There are many aspects of this approach that are attractive. Those of this persuasion will, of course, think that the answer to the question 'do our world-oriented uses of mathematics justify any kind of realism?' is straight-forwardly 'no'. None of this, however, speaks against the development of descriptive accounts of mathematical explanation and representation: Maddy's naturalist should be interested in this project as an investigation into the kinds of things that we do with mathematics and how we do them.

[3] Here's this approach captured in a slogan: "Metaphysics so revived does not bother asking whether properties, meanings, and numbers exist. Of course they do! The question is whether or not they are fundamental" (Shaffer 2009: 347). See also Correia & Schieder 2012. This is not to say that easy arguments are *only* offered by those who are part of the program that Shaffer characterises: Thomasson, I take it, thinks that existence questions are worth asking *and* that easy arguments are successful ways of answering them (Thomasson 2013).

0.3. Anti-exceptionalism as a working hypothesis

The second theme running through the thesis is largely orthogonal to questions about mathematical ontology. This concerns questions about how mathematical explanation and representation relate to other forms of explanation and representation.

I will adopt, as a working hypothesis, anti-exceptionalism about mathematical explanation and mathematical representation.[4] Exceptionalism about mathematical representation is the view that the phenomena of mathematical scientific representation is such that it cannot be understood using our current best theories of representation, with exceptionalism about mathematical explanation glossed analogously. Adopting anti-exceptionalism as a hypothesis to be tested consists in asking: can the special mathematical cases of explanation and representation be helpfully understood by appealing to current theories of explanation and representation? I argue that, contra the assumptions of many of the commentators discussed in this thesis, there is reason to believe that the anti-exceptionalist hypothesis is correct. To be an anti-exceptionalist is not to deny that there are *any* idiosyncratic features of the phenomena that one is anti-exceptionalist about: holding that explanation in both biology and chemistry ultimately involves describing the world's causal structure does not commit one to thinking that biological causal explanation and chemical causal explanation function *identically*. By identifying the common core between mathematical and non-mathematical explanation and between mathematical and non-mathematical representation, light can also be shed on where these types of explanation and representation differ.

Exceptionalism and anti-exceptionalism are views about a particular kind of explanation (or representation) and their truth is determined by the relationships between the kind of explanation (or representation) under consideration and other kinds, and accounts of, explanation (or representation). One might be an anti-exceptionalist about extra-mathematical explanation but an exceptionalist about normative explanation, an anti-exceptionalist about theory choice in logic but exceptionalist about theory choice in metaphysics, and so on. A second kind of view concerns explanation (or representation, or theory choice, etc.) *simpliciter*. These views address the question of whether or not there is a *single*, fully general, account to be given of all the practices that we call 'explanation' (or

[4] Other anti-exceptionalist projects include anti-exceptionalism about theory choice in logic, according to which (roughly) the norms of theory choice in logic are also those that govern theory choice in scientific disciplines (Hjortland 2017; Maddy 2002; Williamson 2013b; Williamson 2017).

'representation', or 'theory choice', etc.). This much broader question cannot, and so will not, be resolved in this thesis. Nevertheless, the anti-exceptionalist accounts offered of mathematical explanation and representation do offer more support for some of these general views than they do for others. This picture will emerge once the details of the views are spelled out and I will return to this question in more detail in the conclusion.

0.4. The applicability of mathematics

Here is one final positioning of this thesis in an existing debate and literature. Part of this thesis is concerned with understanding mathematical scientific representation. How does this relate to the long-standing problem of what Wigner calls the unreasonable effectiveness of mathematics (Wigner 1960)? The project of chapters 5 and 6 should be understood as addressing something of a more basic question than Wigner's. The question of the unreasonable effectiveness of mathematics takes as data *that* there are very many successful uses of mathematics and, often combining this with historical claims about the distinct development of the mathematical tools and their subsequent deployment in scientific practice, asks for an explanation as to why this seemingly unreasonable thing has occurred. Developing an account of *what it is* for a piece of mathematics to count as a representation of something non-mathematical is largely orthogonal to whether or not it is *unreasonable* that successful mathematical representations like these are commonplace. I am inclined towards the view that the perceived unreasonableness of mathematics ought to be tempered both by the existence of many *un*successful applications of mathematics and by the fact that whether or not it is surprising, unreasonable, or in demand of an explanation that mathematics ends up being effective turns on what our priors are.[5] What is important is that questions about what makes it such that a piece of mathematics represents something non-mathematical is largely orthogonal, and prior, to whether or not it is miraculous, or unreasonable, that this is so. I will, accordingly, say very little about unreasonableness in this thesis.

[5] Bangu and Pincock have each recently developed this first idea in two different ways (Bangu 2016: 26-28; Pincock 2012: 169-190).

0.5. Chapter précis

Chapter 1: Indispensability, representation and explanation

In the first chapter, I provide the recent philosophical context in which this thesis' focus on explanation and representation is situated. In doing so, the aim is to further motivate the paying of closer attention to the way in which mathematical explanation and mathematical representation function. According to the Quine-Putnam indispensability argument, the fact that our best scientific theories indispensably contain terms that purport to refer to mathematical objects is sufficient to justify belief in such objects. I discuss a line of response, pressed by, amongst others, Penelope Maddy and Joseph Melia. According to this response, the standard indispensability argument fails because it does not to distinguish between the various roles that mathematics plays in our best scientific theories and it is reasonable to think that only some roles are ontologically committing. This sets the scene for the rest of the thesis, in which two particular roles played by mathematics in our best scientific theories are interrogated: explanation and representation.

Chapter 2: The enhanced indispensability argument and inferential conservativeness

In the second chapter I begin the discussion of the enhanced indispensability argument, an argument that appeals to the purported *explanatory* capacities of mathematics. This chapter constitutes an argument for the claim that the enhanced indispensability argument, cannot be assessed without an account of how mathematics plays this explanatory role. This claim may, of course, be quite fairly described as common-sense. However, the mathematical realist claims to offer reasons for thinking that their opponent is *already* committed to thinking that the inference from the existence of mathematical explanations of non-mathematical facts to mathematical realism is licit in virtue of some of their prior commitments. I consider two arguments (the argument from inferential conservativeness and the argument from abductive maximalism) along this line and argue that they both fail. I then set the scene for chapter 3 by discussing some canonical examples of extra-mathematical explanation and some criteria by which we can assess putative accounts.

Chapter 3: Extant accounts of extra-mathematical explanation

In the third chapter, I appeal to the criteria and case studies introduced at the end of the second chapter to evaluate existing accounts of extra-mathematical explanation in the recent literature. I first discuss two accounts of extra-mathematical explanation that are *anti-exceptionalist* in character: that is, attempts to extend existing accounts of explanation to the extra-mathematical cases. I first consider Baker's schematic extension of the deductive-nomological account of explanation and Lyon's extension of Jackson and Pettit's programming account of explanation. I then discuss two more recent potential accounts of extra-mathematical explanation that are exceptionalist in character. I consider the abstract dependence account, recently proposed by Pincock and the constraint account, proposed by Lange. I argue that although both accounts succeed in capturing some of the distinctive features of extra-mathematical explanation, they are ultimately problematic.

Chapter 4: The modal account of extra-mathematical explanation

In the fourth chapter, I motivate and endorse a positive proposal. I first discuss and discount some reasons for thinking that extending causal accounts of explanation to the extra-mathematical case is a *prima facie* non-starter. After motivating appealing to Woodward's interventionist account of causal explanation, I make salient the relevant features of this account and demonstrate that it can shed light on extra-mathematical explanation. I then draw on forthcoming work by various commentators to argue (in line with Jansson & Saatsi and contra Ripley, Colyvan & Baron, and Chirimuuta) that extending Woodward's account in this way does not require appealing to counterpossibles. I then suggest that appealing to the structural nature of extra-mathematical explanations can explain *in virtue of what* a mathematical fact can play the same role played by the invariant generalization in paradigm causal explanations. Before concluding, I consider the causal status of extra-mathematical explanations.

Chapter 5: Scientific representation and epistemic representation

In the fifth chapter, I begin my discussion of the ways in which we use mathematics to represent by introducing some case studies of both mathematical and non-mathematical scientific representations. I then address some questions about scientific representation more generally. In particular, I consider two challenges found in the literature on scientific representation that threaten to undermine the accounts considered and assessed in chapter 6. I first consider Callender and Cohen's claim that there is no special problem of scientific representation (and *a fortiori* no special problem of mathematical scientific representation). I second consider Suárez and Frigg's challenges for accounts of representation that give a role to structural relations between vehicle and target (which, to give a minor spoiler, includes the accounts of mathematical representation discussed in chapter 6). I argue that the Suárez-Frigg objections can be avoided if accounts of representation make use of structural relations to account for the degree of faithfulness of a representation and not to explain the vehicle's status *as a representation*. I suggest that a form of Contessa's epistemic representation account has these features. The concluding picture is one in which a vehicle represents its target in virtue of standing in a (perhaps basic) denotation relation with its target, a vehicle is an epistemic representation of its target in virtue of a user providing an interpretation of the target in terms of vehicle objects and a vehicle is a faithful epistemic representation in virtue of standing in a structural relation with its target.

Chapter 6: Distinctively mathematical epistemic representation

In the sixth chapter, I consider accounts of distinctively mathematical scientific representation: Pincock's mapping account and Bueno and Colyvan's inferential conception. I argue for the anti-exceptionalist conclusion that mathematical representation can be accommodated by the epistemic representation framework, set out in chapter 5. I set out and offer solutions to two potential problems with such an accommodation, and also suggest that (contra Bueno and Colyvan) the mapping account and inferential conception share a common core and may be complementary. The resulting picture of representation is as follows: scientific representation is a subspecies of (partially) faithful epistemic representation and distinctively mathematical scientific representation is a subspecies of scientific representation. Distinctively mathematical scientific representation is therefore a form of epistemic representation: as a result, anti-exceptionalism about distinctively mathematical representation is true. I then discuss the metaphysical ramifications of the structural account of mathematical representation: in particular, I appeal to notions of epistemic understanding to argue against Pincock's claim that in order for one to *understand* a mathematical representation, one must believe its mathematical content.

In the conclusion, I return to the themes of mathematical ontology and anti-exceptionalism with the discussion of the six chapters in mind, as well as signalling the possibility of future work.

Chapter 1

Indispensability, representation and explanation

In this chapter, I provide the recent philosophical context in which this thesis' focus on explanation and representation is situated. In doing so, the aim is to further motivate the paying of closer attention to the way in which mathematical explanation and mathematical representation function. According to the Quine-Putnam indispensability argument, the (purported) fact that our best scientific theories indispensably contain terms that refer to mathematical objects is sufficient to justify belief in such objects. I discuss a line of response, pressed by, amongst others, Penelope Maddy and Joseph Melia. According to this response, the standard indispensability argument fails because it does not to distinguish between the various roles that mathematics plays in our best scientific theories and it is reasonable to think that only some roles are ontologically committing. This sets the scene for the rest of the thesis, in which two particular roles played by mathematics in our best scientific theories are interrogated: explanation and representation.

In the first section I make some brief historical remarks about what has come to be known as the Quine-Putnam indispensability argument and discuss various formulations of the argument. In the second section I discuss Field's programme and place it in the context of this thesis. In section three I will present a formulation of a recent response to the indispensability argument, owing to Maddy and Melia amongst others, according to which the argument fails because it does not distinguish between the various roles that apparent reference to mathematical objects plays in our scientific theories. In section four I argue that the success of the Maddy/Melia response to the indispensability argument depends on the nature of the explanatory and representational roles played by apparent reference to mathematical objects. I attempt to make precise the component views that make up representationalist nominalism and make the case that descriptive accounts of mathematical explanation and representation must be developed if the view is to be assessed. In section five I respond to a challenge to the kind of view discussed in section four, owing (in different formulations) to Colyvan and Pincock. I then conclude.

1.1. The Quine-Putnam indispensability argument

In this section I set out (what has come to be known as) the Quine-Putnam indispensability argument.

Consider the following explication of the indispensability argument, owing to Mark Colyvan:

> (1C) We ought to have ontological commitment to all and only those entities that are indispensable to our best scientific theories.
> (2C) Mathematical entities are indispensable to our best scientific theories.
> Therefore
> (3C) We ought to have ontological commitment to mathematical entities. (Colyvan 2001: 12)

Something like this argument is often referred to as the Quine-Putnam indispensability argument and ascribed to one or other (or both) of Quine and Putnam (Field 1980: 107; Maddy 1990b: 29-30; Shapiro 1997: 46; Melia 2000: 45; Yablo 2000: 197; *cf.* Liggins 2008). Whether or not the argument referred to as the Quine-Putnam indispensability argument is something that would be endorsed by either Quine or Putnam is a fraught exegetical question. I'll briefly touch on this question here, and then explain why it will not pursued in any depth in this thesis.

The conclusion of the above argument has as its conclusion what I referred to in the introduction as Platonism. As Liggins persuasively argues, for this reason, the ascription of the above argument to Putnam is a misreading (Liggins 2008: 119-123).[6] The core of this misreading is that Putnam has rather different approaches to the notions of ontological commitment and realism to those presupposed by the above argument: I will briefly comment on each in turn. Putnam's notion of realism, at least that appealed to in one of his presentations of (what looks like) the indispensability argument, contrasts with *both* Platonism (understood as the view that mathematical objects exist) and truth-value realism (understood as the view that (at least some) mathematical sentences are (non-trivially) true). Putnam's notion of realism appeals to notions of objectivity – the notion that mathematical sentences are bivalent and that what determines the truth-value of a given mathematical

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[6] My discussion in this section follows Liggins' presentation: see Liggins 2008 for further details and references.

sentence is "something external […] not (in general) our sense data, actual or potential, or the structure of our minds, or our language, etc." (Putnam 1975: 70). As Liggins notes after quoting this same passage, *this* notion of realism cuts across both Platonism and truth-value realism. Consider a view that holds that the truth-value of a mathematical sentence is determined by abstract truth-makers, that there *are* no such abstract truth-makers and that, therefore, all mathematical sentences are false.[7] This view is non-Platonist, non-truth-value-realist but realist in Putnam's sense in that the truth of mathematical sentences is determined by "something external". Hartry Field, for example, is a realist in Putnam's sense but is taken to be a canonical *non*-realist in the debate surrounding the indispensability argument.

Liggins also provides reasons for thinking that the standard presentations of the argument ascribe to Quine a Davidsonian argument that he would not have endorsed (Liggins 2008: 118-119). However, there is an (additional) worry that the ascription of the above argument to Quine is guilty of the same flattening of Quine's views about ontology that is often found in the contemporary metaphysics literature. This is a version of Quine that is perhaps found in 'On What There Is' (Quine 1953) but largely absent from the Quine of 'Ontological Relativity' (Quine 1963). According to the standard story, Quine revived the traditional ontological project from its Carnapian condemnation (Carnap 1950; Quine 1953) and we should understand a properly Quinean approach to ontology as involving using our standard first-order logic (with identity) to regiment our scientific theories and reading off those objects that are quantified over. This sort of picture seems to be both implicitly and explicitly supposed in much work in metaphysics and is also the object of some discontent in the metametaphysics literature. This discontent ranges from defences of a broadly Carnapian picture against Quinean attacks (Price 2009) to arguments that Quine's views are misrepresented (Nay 2012; Soames 2009; Wilson 2011).

What should be taken from this metametaphysical debate is that it is controversial to claim that Quine held that we should, and can, give an answer to the questions of the form 'does x exist?' by finding out whether or not x is indispensable to (a suitable regimentation of) our best scientific theories.[8] In this thesis, I will not engage in any deeper exegetical discussion of whether or not either Quine or Putnam endorsed any argument resembling what has come to be known as the Quine-Putnam indispensability argument. Those interested in this

[7] Being slightly more careful: some mathematical sentences, on this view, will be trivially true (such as 'there are no prime numbers between 11 and 13').

[8] Indeed, the above argument doesn't even make appeal to the notion of regimentation.

narrower question should pursue discussion elsewhere (Resknik 1997; Liggins 2008) and those interested in the faithfulness of the neo-Quinean project in contemporary analytic metaphysics to Quine's actual thought should consider the references above. This disregarding of exegetical questions is motivated by the fact that discussions of this version of the argument in literature over the past couple of decades take as their starting point *something like* Colyvan's formulation, with consideration of Quine and Putnam's views being a secondary consideration, if they are featured at all.[9] The move, then, is from an investigation of Quine's argument to an investigation of an argument with broadly Quinean premises and motivations.

The disconnect from Quine's thought is not complete, however. When defending the premises of the above argument, realists often appeal to Quinean notions – even if this is, ultimately, distinct from the above argument being one that Quine would endorse. (1C) requires unpacking and Colyvan does so by appealing to two Quinean notions, stating that "the crucial first premise follows from the doctrines of naturalism and holism" (Colyvan 2001: 12). Leng's formulation of the argument that precedes her critical discussion makes the incorporation of these Quinean notions explicit:

> (1L) (Naturalism): We should look to science, and in particular the statements that are considered best confirmed according to our ordinary scientific standards, to discover what we ought to believe.
> (2L) (Confirmational Holism): The confirmation our theories receive extends to all their statements equally.
> (3L) (Indispensability): Statements whose truth would require the existence of mathematical objects are indispensable in formulating our best-confirmed scientific theories.
> (C) (Mathematical realism): We ought to believe that there are mathematical objects. (Leng 2010: 7).

There are still open questions as to whether or not this is an argument that Quine would endorse, even though this formulation makes explicit the fact that it depends on views central to Quine's thought. I take (3L) and (2C) to have identical truth-conditions despite their

[9] This move from arguments originally given in their original philosophical contexts to somewhat flattened versions of those arguments, which then are discussed and responded to and become canonical, is not unique to the Quine-Putnam indispensability argument (see, for example, discussion of philosophical arguments that, purportedly, do not appeal to intuitions in their original presentations yet have entered the literature in modified forms such that they do (Cappelen 2012)).

different formulations: I will mention them interchangeably and take 'the indispensability premise' to refer to either or both. I also take (1C) to be motivated by an acceptance of (1L) and (2L), as Colyvan makes explicit (Colyvan 2001: 18).

1.2. Field's hard-road response and the indispensability premise

In this section I discuss Hartry Field's response to the Quine-Putnam indispensability argument. Whilst I hope to say something interesting about the modal character of the indispensability claim, the primary purposes of discussing Field's programme are the following. First, it is a frequent response to views and arguments in this area that endorsing those views and arguments commits one to carrying out (something like) Field's programme – in order for this claims to make sense, it is necessary to have in mind what Field's programme entails and why it is a disadvantage of views if they are committed to carrying it out. Responses of this kind tend not to make the strong assumption that carrying out Field's programme is impossible – rather, it is merely that having to demonstrate that the programme *can* be carried out is a hefty theoretical burden. Second, the Maddy-Melia manoeuvre that is the focus of the next section sharply contrasts with Field's approach, even though both (in their own ways) involve rejecting the indispensability argument – it is necessary to have Field's approach in mind to make this distinction clear.

Hartry Field attempts to cast doubt on the indispensability premise (Field 1980; Field 1989). Field's programme is an example of a nominalization programme: such programmes aim to systematically demonstrate that it is possible to substitute the mentions of mathematical objects in our best scientific theories for terms that purportedly refer to some other, supposedly less nominalistically-worrying kind of object. If mathematics is dispensable to our best scientific theories, then for each mathematized scientific theory there exists a nominalistic counterpart, which is just as good as the mathematized theory (with regards to predictive power, simplicity, and so on) yet only contains terms that purport to refer to nominalistically acceptable objects. Field's programme makes use of space-time points (Field 1980), whilst Chihara appeals to what can be constructed (Chihara 1990) and Hellman to what structures might have been (Hellman 1989). Field does not offer a nominalization of our best scientific theories (circa 1980): rather, he proposes a way to nominalize Newtonian gravitational theory, with the hope that doing so both decreases our credence in the claim that mathematics is indispensable and offers tools for nominalizing some of our current best science.

Some philosophical problems with such nominalization programmes are shared amongst all of the programmes: worries, for example, about whether nominalized counterparts are as theoretically virtuous as their mathematical counterparts, even if they are empirically equivalent (Colyvan 2001: 78-79) and worries about the nominalist credentials of the objects and properties appealed to in lieu of the mathematical objects (Burgess & Rosen 1997: 140; Leng 2010: 57-62). Technical worries about the details of such programmes are likely to be more diverse. There are many good surveys of problems with such nominalization programmes: see Rosen & Burgess 1997 for a thorough overview that discusses Field, Chihara and Hellman, and for discussions of Field's programme see MacBride 1999 and Leng 2010: 45-70. It is also important to note the recent work that has been done in this vein since the publication of *Science without Numbers*. For example, Cian Dorr and Frank Arntzenius' offer an extension to non-Newtonian space-time theories and fiber-bundle theories (Arntzenius & Dorr 2012). A frequently expressed worry about nominalization programs concerns their extension to quantum mechanics (Malament 1982) but recent work, too, has been done on casting doubt on this worry (Keming-Chen *ms*).

In addition to completing a nominalization programme (or at least giving us good reasons for thinking that one can be completed), this brand of response to the indispensability argument also attempts to explain why it is unproblematic (and in fact useful) to include in our scientific theories terms that purport to refer to objects that, in fact, fail to exist. Field argues in favour of the claim that mathematics is conservative. Let a theory T' be a conservative extension of another theory T iff:

(a) the language of T' extends the language of T
(b) every theorem of T is a theorem of T'
(c) any theorem of T' that is not a theorem of T is not expressible in the language of T

A mathematized scientific theory, then, is claimed to be a conservative extension of its nominalistic counterpart.[10] Field offers both formal and informal arguments for the claim

[10] Field's claim is that mathematics is *semantically* conservative over nominalistic theories.

that mathematics is conservative (Field 1980: 115). For discussion of potential problems with the set-theoretic proof of the claim see (MacBride 1999: 448-449) and for an argument that the proof, even if successful, cannot do the work that Field requires of it, see Melia 2006.

As has only been signalled above, there is extensive critical discussion of Field's attempt to nominalize Newtonian gravitational theory, his attempt to provide good reasons to believe the more general claim about dispensability and his attempt to demonstrate the truth of semantic conservativeness. Rather than interrogate any of these critical responses and what Field might say in response (a task already carried out in detail in the extant literature signposted above), it is worthwhile instead to take stock and ask exactly what this uncertainty means as regards our credence in the indispensability premise of the indispensability argument.

(3L)/(2C), I take it, is supposed to be something other than a mere descriptive claim about our current failure to complete a nominalization programme. Despite recent work, this much is uncontroversially true: even if Field has successfully nominalized NGT, this is not part of our current best science and even if Dorr and Arntzenius, and Keming-Chen, are successful in their efforts, this makes up only a fraction of our current best science. But, of course, the indispensability premise does not just state that our current best theories are not yet nominalized (although its truth would entail that this will remain the case!). Rather, the premise makes the stronger claim that our best scientific theories are indispensably mathematized: they are unnominaliz*able* rather than currently unnominzaliz*ed*. It is difficult to find explicit arguments for this modal claim. One strand that can be identified is that the current failure to complete the nominalization task is supposed to feature in the argument for (3L)(2C) (Colyvan 2001: 89). The standard explications of the notions of indispensability only push the question back. For example, Colyvan says that he takes:

> 'Dispensability' to be defined as follows: [A]n entity is dispensable to a theory if there exists a modification of that theory resulting in a second theory, functionally equivalent to the first, in which the entity in question is neither mentioned nor predicted. Furthermore, the second theory must be preferable to the first. (Colyvan 1998: 40)

The same ambiguity between nominalizable and nominalized is present in Colyvan's invocation of the notion of there existing a modification of all our scientific theories (of what I have been referring to as a nominalistic counterpart). If the truth conditions of there being nominalistic counterparts to our best scientific theories is that we have such counterparts to hand, as it were, then it is false that such counterparts exist. But this, of course, is an intentional misreading. What is of relevance is whether or not it is *possible* to explicitly formulate such theories, in virtue of these theories existing in some weaker sense.

What could ground this inference from the current failure of nominalization tasks to the stronger modal claim? It should be immediately clear that an inference from the failure to complete a task T to the claim that it is not possible (for some strong species of possibility) to complete T is not one that we normally take to be licit. The fact that in 1900 no-one had completed the task of building a rocket to the moon would not have licensed an inference to the claim that no-one *could*. Nor does it seem viable to say that inferences of this kind are permissible if we narrow the scope to formal or mathematical tasks. The move from the failure to complete some technical or mathematical task T to the claim that it is not possible to complete T is not one that we commonly make.

Perhaps the argument for the modal claim does not rely on a straightforward inference from the failure of completion. Rather, the argument might be that it is the *best explanation* for the fact that the task has not been completed that it cannot be completed, in virtue of our best theories being unnominaliz*able*. One could propose a disanalogy between the nominalization task and the rocket task. For the rocket task, the best explanation for the failure of its completion would have been a set of contingent facts about technological and scientific progress. However, the nominalist can offer a very good competing explanation for the failure to complete the nominalization task. First, completing the task is likely to be incredibly difficult. Secondly, it should be noted that those most suited to completing the nominalization task are not aiming to do so:

> This open research problem is moreover one that has so far been investigated only by amateurs – philosophers and logicians – not professionals – geometers and physicists; and the failure of amateurs to surmount the obstacles is no strong grounds for pessimism about what would be achieved by professionals. (Burgess & Rosen 1997: 70)[11]

[11] It is the view, at least of Burgess I imagine, that the very fact that no "professionals" are carrying out the open research problem counts as evidence that it is misplaced.

Even though Burgess and Rosen are concerned with drawing out problems with current attempts to tackle the nominalization task, they too seem to admit that the current failure of attempts to do so provides no strong evidence for the strong claim that the task cannot be carried out. Such a response should be tempered, of course, by remembering that (at least some of) the problems that face nominalization programmes are philosophical, rather than technical: such as the delineation of posits of nominalized theories between nominalistically acceptable and nominalistically unacceptable.[12]

What should our considered stance be towards nominalization programmes? Nothing I have said in this section casts doubt on the difficulties (both technical and philosophical) that face nominalization programmes. However, I think it is less than clear what our resulting credence ought to be in (3L/2C). It is clear that we ought not be confident that a nominalization programme can be carried out – doubt has been cast that *any* of our best scientific theories have been successfully nominalized. However, it is also not clear that we should be confident that the strong modal claim that our theories *cannot* be nominalized is true either: the inference from the current failure to the necessity of failure is shaky.

How to resolve this problem of unclear credence in the indispensability premise is not straightforward. If, as the argument says when read strictly, we should believe in objects that are genuinely indispensable to our best scientific theories then we should reserve judgement about our ontological commitments if we should reserve judgement about claims about indispensability, as it seems we should. If, as the realist might suggest in response, we should affirm the existence of any objects that are purportedly referred to by our *current* scientific theories, then any notion of indispensability drops out of the picture: whether or not such reference is dispensable, our current best scientific theories certainly *do* currently contain terms that purport to refer to mathematical entities.

This is all to say that there are additional reasons to trying to address the indispensability argument differently, in addition to the primary reason standardly given for moving away from arguments about dispensability, which is discussed in the next section.

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12 The distinction between outstanding technical difficulties and philosophical difficulties is not a sharp one.

1.3. The Maddy-Melia response

In this section I discuss a recent prominent response to the Quine-Putnam indispensability argument that do not turn on the viability of a nominalization programme.

According to (what I will call) the Maddy-Melia response, it is insufficient for the realist's needs that terms that purportedly refer to mathematical objects indispensably feature in our best scientific theories. Rather, it must be the case that the mathematical objects purportedly referred to by these terms play a particular kind of role: a role roughly analogous to that played by unobservable physical objects.[13] Considerations of indispensability *simpliciter* play a less prominent role: if the Maddy-Melia response is correct, then it is necessary but insufficient that terms that purport to refer to mathematical objects are indispensable to formulating our best scientific theories. If such objects play a different, lesser, role than that of unobservable physical objects, then terms referring to these objects can be indispensable without this being ontologically committing. (The notion of indispensability remains important, of course, in the sense that if (something like) Field's programme can be completed, then mathematical objects do not indispensably play *any* role in our scientific theories).

That an object can be purportedly referred to by a term that is indispensable to one of our best scientific theories, and yet rationally not believed in, gained traction after Maddy introduced an influential objection to the indispensability argument.[14] An important part of this move is the rejection of confirmational holism. Across two papers (Maddy 1992; Maddy 1994), Maddy sets out an example that demonstrates that an object can be appealed to in our best explanations of some observed phenomena without this motivating belief in that object from practising scientists. Maddy notes that in 1860, the atom became "the fundamental unit of chemistry" (Maddy 1994: 394), whilst it was not until 1913 that it was "accepted as real" (*ibid*): Colyvan notes that "renowned scientists such as Poincaré and Ostwald remained sceptical of the reality of atoms until as late as 1904" (Colyvan 2001: 92). In addition to this

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[13] This is motivated by the assumption that all interlocutors are scientific realists, of a certain kind. I discuss this assumption in much more depth in chapter 2.

[14] It is important not to infer from this that Maddy is a nominalist. Various time-slices of Maddy have been realists of various kinds, for various reasons. This ranges from realism on the basis of the possibility of set-perception combined with Quinean motivations (Maddy 1990a), to a form of realism on the basis of a kind of naturalism with respect to mathematics as opposed to science (Maddy 1997), to her most current stance that both that nominalism (or what she calls Arealism) and Thin Realism (an idiosyncratic kind of truth-value realism) are consistent with mathematical practice, or 'the facts of mathematical depth' (Maddy 2011).

case, Leng (via Maddy) discusses appeals in scientific theories to so-called 'idealised objects', such as continuous fluids and frictionless planes (Leng 2002: 399; Maddy 1992). The standard suggestion is that purported reference to such objects can be useful, but not true:

> If we remain true to our naturalistic principles, we must allow a distinction to be drawn between parts of a theory that are true and parts that are merely useful. We must even allow that the merely useful parts might in fact be indispensable, in the sense that no equally good theory of the same phenomena does without them. Granting all this, the indispensability of mathematics in well-confirmed scientific theories no longer serves to establish its truth. (Maddy 1992: 281)

That our theories indispensably contain terms that purportedly refer to objects, such that commitment to the existence of objects is not secured by this fact, does not tell us much about what these different kinds of roles are, nor how to sharply distinguish the two sets of objects. There is, also, controversy about whether or not confirmational holism is required for the indispensability argument, exactly what confirmational holism comes to (Morrison 2017) and whether or not any version of confirmational holism is both plausibly true and able to play the role required for the indispensability argument (Field 2016: P-31; Morrison 2011).

There is reason to think, though, that the core insight of the examples given by Maddy and Leng (and others) comes apart from questions about the precise formulation of confirmational holism and its role in the indispensability argument. The insight seems to be that not all objects purportedly referred to by the terms of our scientific theories play the same kind of role and, as the examples attempt to illustrate, it is an open question as to whether or not *all* of these roles are such that a posit playing this role generates ontological commitment to that posit. For these reasons, recent debate has moved away both from the plausibility of the indispensability premise and the confirmational holism premise to asking something of a *comparative* question. This question is: do posited mathematical objects play the *same* kind of role in our scientific theories as unobservable physical objects (or: does the use of terms that purportedly refer to mathematical objects play the same kind of role in our scientific theories as the use of terms that purportedly refer to unobservable physical objects)? It is the answer to this question that, recently, has been taken to fix Platonism's fate.

A recent non-Fieldian nominalist view answers 'no' to this question: mathematical objects play a *merely* representational role in our scientific theories, perhaps much as idealized objects are apparently appealed to in order to express facts about the objects that are the genuine subject matter of our theories. This response – which I will refer to as 'representationalist nominalism' and discuss in more detail in the next section – claims that this contrast in roles played by (purported reference to) mathematical objects and unobservable physical objects entails a contrast in the ontological attitude we ought to have to these objects. A Platonist answer to the question is: 'yes' – both mathematical objects *and* unobservable physical objects play a role in explanation and, furthermore, explanation is linked to existence in the appropriate way.

1.4. The roles of mathematics

In this section I draw out the ways in which the notions of explanation and representation have been appealed to by interlocutors on either side of the debate. I make the case that the debate can be aided by the development of accounts of mathematical explanation and mathematical representation, the tasks carried out in the remainder of the thesis.

What, then, is the role that mathematics has, taken to be strongly disanalogous to the role played by terms that purport to refer to unobservable physical objects? The view has been expressed in a diverse number of ways. One way of understanding it is to take mathematics as something like a language, or framework, in which a theory's content is stated or expressed: much as we might express a proposition by using a natural language, we express content about the world using mathematics (Field 2016: P-33). On this understanding, the disanalogy between unobservable physical objects and mathematical objects is stark: the former constitute the subject matter of our scientific theories whilst the latter are mere units of a language with which facts about this subject matter are expressed. A second way of understanding this role is to appeal to notions of indexing. This is a common way of understanding the role in the literature (Daly & Langford 2009; Melia 2000; Melia 2002). On this line, mathematics plays the role of pointing to physical facts, or to parts of the non-mathematical world.[15] This contrasts with the role played by unobservable physical objects, which do more than index parts of the world: they *constitute* facts, stand in *causal* relations, *explain* why bits of the world are as they are, and so on. A third way of understanding the

[15] 'Pointing to' is not much less vague then 'indexing'.

role is by characterising it as representational (e.g., Saatsi 2011). On this way of understanding the role, mathematics represents non-mathematical facts: whilst unobservable physical objects stand in causal and explanatory relationships to the world, the mathematical content stands only in a representational relationship to the world. There is reason to think that these three understandings are three ways of coming to understand the same phenomena: indexing, as discussed by Daly and Langford, and Melia, seems to have much in common with denotation, which in turn has much in common with the idea of mathematics being used as a language with which non-mathematical facts are expressed (in as much as we take the words of a language to denote their referents) – it is standard, also, to talk of language as representing the world.[16]

1.4.1. Explanation

The most prominent realist response to the Maddy-Melia manoeuvre is to accept that it is insufficient that mathematical objects are purportedly referred to by terms indispensably appearing in our best scientific theories, but to then press that mathematical objects *do* in fact play the kind of role that licenses belief in these objects. This is a rejection of the view alluded to above: that mathematics only plays an expressive, indexing or representational role. On this line, there is in fact a strong analogy between the roles played by unobservable physical entities and mathematical entities: both figure in our best explanations of observable phenomena (Baker 2005; Baker 2009; Bangu 2008; Bangu 2012; Lyon & Colyvan 2008: Lyon 2011) On this line, even if it is permissible (in virtue of a denial of confirmational holism) to affirm the existence of some objects appealed to by our best scientific theories and not others, it is *not* permissible to affirm the existence of unobservable physical objects whilst rejecting mathematical objects, in virtue of these two kinds of objects playing the *same* kind of role in our scientific theories. Contra the views discussed above, mathematics is used to do *more* than express non-mathematical facts. This line of thinking results in the enhanced, or explanatory, indispensability argument, discussed in detail in chapter 2. The core of the debate becomes whether or not there are any cases of mathematical objects playing an explanatory role in the same sense that unobservable physical objects do, and whether the existence of such explanations genuinely licenses belief in mathematical objects in the same way that the existence of explanations of observable phenomena that appeal to unobservable

[16] In §5.4 I set out the distinctions between representation *qua* denotation and other kinds of representation in detail.

physical objects (is purported to) license inference to the latter objects. The various components of this move in the debate are the focus of the next three chapters. For now, we can understand this view as consisting in Explanationism (the view that mathematics sometimes plays an explanatory role in our scientific theories) and the Explanation Conditional (the view that if mathematics sometimes plays an explanatory role in our scientific theories, then some form of mathematical realism is justified). Before concluding and beginning the discussion of mathematical explanation, in the next section I attempt to make precise the representationalist nominalist view gestured at above.

1.4.2. Representation

As noted above, the bifurcation of roles played by different objects in our scientific theories is often coupled with the claim that *if* mathematics only plays a representational or indexing role in our scientific theories *then* our world-oriented use of mathematics does not support realism about mathematics. Baker refers to these claims as *representationalist nominalism* whilst Liggins refers to the claim that mathematics plays an indexing/representational role in science (and *not* an explanatory one) as abstract expressionism (Liggins 2014: 600). For ease of reference, I will introduce two formulations of these claims:

> **Representationalism**: Mathematics merely plays a representational role in our scientific theories.[17]

> **Representation Conditional:** If mathematics merely plays a representational role in our scientific theories, then our world-oriented uses of mathematics do not justify mathematical realism (of some kind).

Let representationalist nominalism, then, just be the conjunction of Representationalism and the Representation Conditional: the claim that, in virtue of the fact that the antecedent of the Representation Conditional is true, our world-oriented uses of mathematics do not justify mathematical realism (of some kind). I'll briefly make some clarificatory remarks about the two components and their combination.

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[17] Representationalism, in this context, has nothing to do with Representationalism understood as a view about the characteristic properties of the mental and representational content (Dretske 1995; Tye 2000).

Representationalism and the Representation Conditional can come apart: one is a claim about the nature of world-oriented uses of mathematics and the other is a claim about what it is fair to infer from the former claim, if it is true. Baker does not explicitly assent to the Representation Conditional, although implicit assent can be read into the fact that he chooses to focus on arguing that mathematics plays an explanatory role, rather than arguing that the role played by mathematics that is agreed upon by all parties is, in fact, ontologically committing.[18] The natural thought behind representationalist nominalism is that it is perfectly possible for mathematics to play a representational role even if mathematical objects do not exist[19] (Liggins 2014; Melia 2000; Saatsi 2011; Yablo 2001; Yablo 2012).[20]

Despite the name 'representationalist nominalism', it should be clear that nominalism about mathematics is not entailed by the combination of the two claims. In order for the two claims to entail nominalism, one would have to add the claim that the *only* good grounds for being a realist about mathematics is to be found in the details of our world-oriented uses of mathematics. Field makes a claim of this sort, when he says that "it becomes clear that there is one and only one serious argument for the existence of mathematical entities, and that is the Quinean argument" (Field 1980) as does Colyvan when he counts himself amongst the "mathematical realists […] who think that indispensability arguments offer the *only* good reason for that realism" (Colyvan 1998: 39). As noted in the introduction, however, in this thesis I do not consider so-called 'easy' arguments, and defending the claim that the prospects of realism rise and fall with indispensability arguments in turn requires defending the view that easy arguments fail.[21]

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[18] In a separate discussion, Colyvan explicitly states that mathematics playing a representational role is consistent with both realism and nominalism (Bueno & Colyvan 2011: 366). I discuss this in chapter 6.

[19] Being more careful: the representationalist nominalist claim must not be that the fact that mathematics merely plays a representational role is a *positive reason* to think that there are no mathematical objects. Rather, proponents ought to be understood as claiming that mathematics playing a representational role is consistent *both* with realism and nominalism. The further step to an endorsement of nominalism requires both an endorsement of something like Ockham's razor (see Leng 2010: 259-260 for an example of this kind of reasoning) and a consideration of easy arguments.

[20] It is important not to take from this that these authors all explicitly endorse nominalist views about mathematics: as stressed above, one can hold the conjunction of Representationalism and Representation Conditional whilst being a realist (for example, if one is a Maddy-style Thin Realist), a nominalist or holding that ontological questions are malformed in some sense. Furthermore, although Representationalism can be read into all of the discussions cited above, some of the authors have made more recent suggestions that make it sound like they, in fact, deny Representationalism but ground their rejection of realism by maintaining that the Explanation Conditional is false.

[21] That easy arguments are unsuccessful may well also be entailed by the kind of Quinean world-view endorsed by someone like Field at the time of writing *Science without Numbers*. In this case, a direct argument wouldn't be given for thinking that easy arguments are bound to be unsuccessful – rather, it would fall out of a general way of approaching ontological questions.

A brief clarification regarding Representationalism. A natural thought is that there is a straightforward sense in which Representationalism *must* be false, and not in the contentious sense in which the realist argues that it is false in virtue of mathematical objects playing an *explanatory* role. Rather, there seem to be many other roles played by purported reference to mathematical objects, and that this is part of the common ground. Mathematics, as emphasised by Field and others, plays a crucial role in facilitating inferences. So, too, in turn, might we think that some of these inferences have as their conclusions predictions, generated by the theory: the role of prediction, the thought goes, is distinct from the role of representation. Again, in turn, these predictions generated (in part) by appealing to mathematics are used to adjust the level of confirmation of our theories and, in turn, our credences in our theories (for a thorough discussion of the role of mathematics in confirmation, see Pincock 2012: 25-41). This, then, is a straightforward argument for thinking that Representationalism is false: mathematics is used to facilitate inferences, to generate predictions, to confirm theories and to help fix our credences, and this list is unlikely to be exhaustive. Therefore, the argument goes, mathematics does not play a merely representational role in our scientific theories.

The representationalist nominalist has a route for accommodating this near-platitude about the various uses of mathematics, however. The representationalist nominalist should concede that mathematics plays these roles, but argue that they are roles that mathematics can play *in virtue* of mathematics playing a representational role. It is in virtue of the fact that mathematics is capable of representing the non-mathematical that it is possible to use it to facilitate inferences to non-mathematical claims and to generate predictions about the non-mathematical. Indeed, one way of understanding representationalist nominalist attempts to accommodate seeming cases of extra-mathematical explanation (for example, the earlier Saatsi (Saatsi 2011)) is to understand it to be the claim that whatever explanatory role mathematics has is parasitic on its representational capacities in the same way.

It is an important aspect of the representationalist nominalist view that Representationalism doesn't imply that mathematics is dispensable: the representationalist nominalist explicitly eschews Field's programme and those like it. (This, then, is why Colyvan refers to Field-type nominalisms as hard road responses and those that don't endorse the dispensability of mathematics as easy road responses (Colyvan 2010)). Rather, such nominalists often accept that mathematics is *indispensable for* the purposes of expressing facts about the world. Such a view, then, does not involve arguing against realism by aiming to demonstrate that mathematics is dispensable: rather, via the Representation Conditional, arguing that even though mathematics is indispensable, it is only used for ontologically innocuous purposes – the purpose of increasing our expressive capabilities. Although Yablo is not, if we are being careful, a nominalist, he captures this aspect of the view well:

> Numbers enable us to make claims which […] we […] would otherwise have trouble putting into words. (Yablo 2002: 230)

For this reason, then, the representationalist nominalist disagrees strongly with Field's claim that, in virtue of the fact that he is a nominalist he must "deny that it is legitimate to use terms that purport to refer to such entities, or variables that purport to range over such entities, in our ultimate account of what the world is really like" (Field 2016: 1). For the representationalist nominalist, our "ultimate account" may well be expressed using mathematics and it may well be *necessary* to do so, but this does not have ontological ramifications.

Whether or not the first component of the representationalist nominalist view is true depends on whether or not mathematics plays an explanatory role: whether or not the antecedent of the Explanation Conditional is true. Mathematics' explanatory role will be discussed in detail in chapters 3 and 4 so I will not discuss this any further here. What about, then, the correctness of the Representation Conditional? It is difficult to find *explicit* defence of this claim in the literature. This is, perhaps, because the representationalist claim is often made in responding to the enhanced indispensability argument: in order to disarm the enhanced indispensability argument, the thought goes, it is sufficient to demonstrate that mathematics only plays a representational role.

This is, of course, correct: if one wants to disarm the claim that mathematical realism is justified on the basis of the obtaining of the Explanation Conditional, then it is sufficient to demonstrate that its antecedent is false. Nevertheless, one might still have worries. Why should we think that it is safe to infer from the fact that mathematics only plays a representational role to the claim that our world-oriented uses of mathematics does not justify any form of realism? Take the following passage from Baker and Colyvan's critical discussion of Daly and Langford's indexing response (which also doubles as a nice example of 'indexing' being taken to come to much the same thing as 'representing'):

> According to [the indexing account], mathematical modelling works in much the same way as map making or any other representational strategy. The basic idea is nicely illustrated in simple cases where mathematics is used to stand proxy for physical properties. The account works well in cases such as those Melia used to motivate it, several of which involve facts expressing distance relations, for example "a is 63 centimetres from b". The indexing strategy takes as its starting point the very natural thought that the above fact does not hold *in virtue* of the relation between *a*, *b* and the number 63; the fact in question is taken to hold by virtue of the spatial relationship between *a* and *b*, and this is all there is to it; this relationship is indexed by the number 63 but the number 63 does not enter into the relationship. (Baker & Colyvan 2011: 324)

Yablo expresses a similar sentiment to the one that Baker and Colyvan ascribe to Daly and Langford:

> The metaphysical issue of whether physical circumstances demand mathematical objects is to be distinguished from the representational issue of what it takes to *state* those physical circumstances. Numbers and functions might indeed be indispensable for this purpose. But so what? (Yablo 2012: 1013)

and suggests that some commentators are guilty of the slide

> from 'we cannot say-without-numbers what a physically complex world would be like' to 'we cannot say what a physically complex world-without-numbers would be like' (*ibid*: 1014)

Let's assume, for the rest of this section, that it is true that mathematics only plays the kind of role described above: that is, let's assume that Representationalism is true and that Daly, Langford and Yablo are correct. On this line, all cases are like the above, where 63 does not enter into the fact being expressed: only *a* and *b* and relations between them, with these relations being taken to be physical relations. But, what about *the fact that* 63 plays a role in representing *a* and *b* and the relations between them? Does 63 enter into *this* fact?

The role being played by 63 is supposed to be intuitively like the relationship between a map and the terrain being represented by the map. The facts expressed by the map (for example, a fact about two roads being parallel to each other) do not hold in virtue of the map (only in virtue of facts about the roads) just as, for the representationalist nominalist, the facts being expressed using mathematics do not hold in virtue of any part of the mathematical realm but only in virtue of how things are with the "physical circumstances", as Yablo has it. But what we are interested in is *the fact that* the map expresses this fact, even if the fact being expressed does not hold in virtue of the map. If pressed to explain the relationship between the map and the terrain being represented, we are likely to tell some vague story about the map and the terrain being similar in various senses: it is these facts about the map and the terrain, and the relations between them, that secures the fact that the map represents the terrain and can express the fact about the roads being parallel to each other.[22] If indexing or representing is supposed to be analogous in this sense, then the realist may claim that we are forced into saying that 63 plays the role gestured at in the above passages in virtue of some of the relations it stands in. The fact being expressed using 63 does not involve 63, but *the fact that* 63 is used to express this fact involves 63. It is important not to slip between these two facts: one is a fact about the world being represented by mathematics, the other is a fact about representation – *about* the representation, or expression, of the former fact using mathematics. A realist wishing to press back on the Representation Conditional, then, will say that surely 63 cannot stand in the kind of relationship it must do in order to be used to express a non-mathematical fact unless it exists: non-existents cannot stand in relations with existents. This suggests a potential non-explanatory route from our world-oriented uses of mathematics to a form of realism: somewhere in-between the indispensability argument and the enhanced indispensability argument.

It seems that the truth of the Representation Conditional seems to currently turn on vague analogies between mathematics and maps *and* on what objects must be like in order to stand in the kind of relationships involved in indexing and representing. In order to adjudicate in a way that is both enlightening and non-question begging, we need to know more about the relationships involved in indexing/representing.[23] In virtue of *what* does 63 express the fact about *a* and *b* and distance relations? Can mathematics play this kind of role without the

[22] I discuss the various senses in which a map represents its target in detail in chapter 4.

[23] To be clear, this worry is very different to that expressed by Baker and Colyvan in their discussion. Their worry is that demonstrating how indexing works in very simple cases tells us nothing about whether or not mathematics is playing this kind of role in the more sophisticated cases found in our scientific theories, with the claim being that this is because in the non-toy cases the mathematics is doing *more than* representing: it is doing some explaining (Baker & Colyvan 2011).

existence of mathematical objects, or the truth of mathematical claims? These questions cannot be answered until more is known about mathematical representation.

Here's a second reason to be agnostic about the truth of the Representation Conditional. Whether or not mathematics' representational capacity justifies some form of realism is an area of disagreement between those developing an account of this role. As mentioned above, Bueno and Colyvan claim that their account of mathematical representation is consistent with both realism and nominalism (Bueno & Colyvan 2011: 366), whilst Pincock suggests that his account requires, or suggests the truth of, a form of realism (Pincock 2012: 217) albeit one that stops short of Platonism.[24] I consider this question in detail in chapter 6: this disagreement is raised here to simply make clear that we are not justified in endorsing the truth of the Representation Conditional without argument. If Pincock is incorrect that mathematics' representational role requires a limited form of realism, reasons for thinking this must be provided by the representationalist nominalist. It seems clear, then, that how we should proceed is by developing an understanding of mathematical representation and adjudicating this disagreement between Bueno & Colyvan and Pincock.

The situation is as follows. Some commentators appear to find it so clear mathematics' representational capacity generates no metaphysical commitments that they do not offer a defence of the claim. Yet, equally obviously, the most natural way of making sense of talk of mathematical objects standing in relations to non-mathematical objects is to take both objects as existing. What is needed to adjudicate this discussion is an account of mathematical representation. This is the subject matter of chapters five and six.

1.5. Potential road-work

Before concluding, it is worthwhile briefly discussing an obvious response to the kind of representationalist nominalist view discussed above. Although the presentation is slightly different, the same basic complaint is made by both Colyvan (Colyvan 2010) and Pincock (Pincock 2012). The complaint proceeds from the observation that the representational nominalist view advises us not to believe our best scientific theories, but to have a different kind of attitude towards them.

[24] As noted above, Pincock's argument from our world-oriented uses of mathematics to a limited form of realism is more complicated than the kind of reasoning found in the indispensability argument and will be discussed in more detail in chapter 6.

Colyvan says the following:

> In short, there are limits to how much weaselling can be tolerated. J.R.R Tolkein could not, for example, late in the *Lord of the Rings* trilogy, take back all mention of hobbits; they are just too central to the story. If Tolkein did retract all mention of hobbits, we could be right to be puzzled about how much of the story prior to the retraction remains, and we would also be right to demand an abridged story – a paraphrase of the hobbitless story thus far. So too for weaselling wherever it arises – at least whenever the weaselling in question is radical enough. The problem we are confronting is that when the weasel oversteps the mark and tries to take back too much (as would be required to purge *The Lord of the Rings* of hobbits, or science of mathematical entities), we no longer have a grip on what is being said. (Colyvan 2010: 295)

It's worth briefly making clear what Colyvan means when he uses the term 'weaselling' when setting out this challenge. Colyvan is targeting Melia's particular brand of representationalist nominalism, which involves the notion of weaselling. To weasel, in this sense, is to make two statements, the second of which 'takes back' some of what is entailed by the first. Melia argues that doing so is commonplace (consider, for example, someone who claims that they 'never drink alcohol, but I do drink exactly one beer on my birthday'). The relevant weasel here is when someone states something that entails the existence of mathematical objects (or states that they believe one of our scientific theories, etc.) but then asserts that they do not believe that mathematical objects exist. (Melia 2000; Melia 2002 for statements of the view and Knowles & Liggins 2016 for a defence of weaselling against some recent objections.) Weaselling is only one way of making sense of mixed-mathematical assertions (like those found in our scientific theories), assuming the truth of representationalism and nominalism about mathematics, and it is not the only way of doing so. However, the central export challenge is more fundamental than merely an objection to Melia's weaselling account and targets any sort of representationalist nominalist view.

Although he does not appeal to Colyvan's earlier explication, in his discussion of fictionalism Pincock raises what seems to be a very similar challenge:

> This "export" challenge is to provide rules that will indicate, for a given context, which claims can be extracted from the fiction and taken literally as claims about the actual world. My argument is that this challenge cannot be met because any set of rules that are detailed

> enough to do the job will presuppose knowledge of the actual world that we do not have. (Pincock 2012: 252)

Here, Pincock is responding to fictionalist accounts. As suggested by the above quote, Pincock considers the possibility of candidate rules for extracting from our scientific theories some claims that ought to be literally believed, but argues that no such set of rules are feasible (Pincock 2012: 252-256). However, there are reasons to think that attempting to offer some set of rules with which we can extract some literal content from our scientific theories is exactly the wrong way to respond to this challenge – so it is unsurprising that Pincock's good faith attempt to carry out this task on behalf of his opponent fails. The export challenge is, in fact, misplaced. As currently characterised, the representationalist nominalist view is that there are some truths about the non-mathematical world that, indispensably, must be expressed using mathematical vocabulary and predicates, combined with the further claim that mathematics playing this expressive (or indexing, or representational) role is consistent with us believing only *what is expressed* using the mathematical vocabulary. Once we are more explicit about the view that Colyvan and Pincock are objecting to, it becomes apparent that the export objection is misplaced. As the quotes above demonstrate, the demand seems to be for the representationalist nominalist to state non-mathematically that nominalistic content that, according to their view, can only be expressed mathematically. If this were possible, then representationalist nominalism would be false. The export challenge is therefore equivalent to stating that a condition on representationalist nominalism's adequacy is that it is demonstrable that representationalist nominalism is false. Liggins nicely suggests that this request is "like responding to the claim that some cases are invisible by demanding to see them all" (Liggins 2012: 999). Another way of demonstrating the unreasonableness of Colyvan's request is to take the (still currently vague) notion of mathematics as a language seriously. The primary purpose of language is to represent, or express, facts about the world. It seems reasonable that, even though we can provide translations into other languages, there is much content that we express using language that we can *only* express using language. Colyvan's request seems akin to demanding that, if we wish to claim that language has this representational capacity as its primary function, we must be able to express *without using language* that which we express using language. Yet, I suggest, this is plainly an unreasonable demand to place on someone making this uncontroversial claim about language. As Yablo says, "the nominalist rejects mathematical *ontology*, not mathematical typology" (Yablo 2012: 1023).

In order for the objection to be a good one, more must be done to motivate the demand that it is possible to state the content of our mathematical theories non-mathematically. It is not clear that Colyvan does much to motivate this. Pincock, however, comes closer to motivating such a demand, when he suggests that the demand that the representationalist nominalist state non-mathematically the content of our best scientific theories is grounded by a more basic demand that we must be told *what it is that we are to believe*. However, representationalist nominalists do often offer such a response: namely, that we should believe that our best scientific theories are nominalistically adequate (Leng 2010: 180; Balaguer 1998: 131). A theory is nominalistically adequate if the theory's nominalistic content is true. The representationalist nominalist advises us, then, not to literally believe our scientific theories, but to believe that their nominalistic content is true. This, then, should be the response to the export challenge. On one understanding, the challenge is question-begging and on another, more nuanced, understanding the representationalist nominalist has offered a response.

There is, of course, outstanding scepticism about whether or not the notions of nominalistic adequacy and nominalistic content can be made sufficiently precise (as Pincock (Pincock 2012: 253) notes). Amongst these are worries about whether or not the notion of nominalistic adequacy can be made precise *without* appealing to objects that are nominalistically unacceptable (such as possible worlds) (Ketland 2011)). There are also, of course, attempts to make the notion more tractable in a range of ways that differ in their degree of precision, such as Balaguer's explication in terms of "the physical world [doing] its part in making [a theory] true" (Balaguer 1998: 135), Rosen's explication in terms of concrete cores and exact intrinsic duplicates (Rosen 2001), Leng's in terms of holding that the "non-mathematical objects are *the way they would have to be* in order for the theory as a whole to be true" (Leng 2010: 199) and attempts to do so by introducing two new modal operators (Pettigrew 2012) or by appealing to the notion of grounding (Liggins 2016).

There are two reasons for thinking that *these* worries should be held sharply apart from the export challenge. First, the worry that the notion of nominalistic adequacy might face its own problems does not mean that the nominalist fails to give an answer to Pincock's non-question-begging version of the export challenge: we are told explicitly what it is that we ought to believe, if not the literal truth of our scientific theories. There are problems with nominalistic adequacy that any complete nominalist account of mathematics will have to address: but, similarly, realists rarely spell out and defend a particular account of truth when they counsel that we ought to literally believe our scientific theories.[25] Second, there's reason to think that some of the prominent problems with nominalistic adequacy are misplaced. It's *prima facie* unclear as to why we ought to find it worrying that the notion of nominalistic adequacy cannot be made precise without appealing to *abstracta* like possible worlds or sentences (if, indeed, it is the case that attempts that explicitly aim to do without such notions (such as Pettrigrew's) fail. The indispensability argument belongs to a tradition of determining our ontological commitments by considering our best scientific theories and, whatever formal features they may or may not have, the explication of philosophical concepts like nominalistic adequacy and nominalistic content are not a part of these best *scientific* theories.

There may well be an argument from the fact that we cannot make the content of a philosophical concept clear without appealing to abstract objects to a form of realism about these objects, but this is distinct from the indispensability argument being considered here. Given that nominalistic adequacy is appealed to in spelling out a view in a debate that has moved past ontological commitment being linked to the mere *mention* of an object of a particular kind, to use this move to lump the nominalist with commitment to possible worlds (assuming, of course, the notion of nominalistic adequacy cannot be explicated without an appeal to such *prima facie* nominalistically-unacceptable objects) seems to be a retrograde step. Perhaps, indeed, there is a form of representationalist nominalism to be had *about* objects like possible words and properties and so on, rather than about mathematical objects! Whether this threatens an unacceptable regress or suggest a unified picture is an open question for the philosopher committed to a thoroughgoing nominalism.

1.6. Conclusion

In this chapter I have traced the indispensability argument through the work of Quine, Putman and Colyvan and argued that in order for the Melia-Maddy manoeuvre to be assessed, the following two tasks must be carried out. First, an account must be given of extra-mathematical explanation, in order to assess the realist's dual claims that mathematics

[25] This fact seems especially striking when, on the Platonist lines, the truthmakers of mixed mathematical-empirical claims are presumably the relations stood in between mathematical and non-mathematical objects.

plays an explanatory role and that this role justifies (some form or other of) mathematical realism. This is the focus of chapters 2, 3 and 4. Second, an account must be given of mathematical scientific representation. This is required in order to assess the claim that the representational capacity of mathematics generates no metaphysical commitments. Developing an account of mathematical scientific representation is the task of chapters 5 and 6. As demonstrated by the easy and natural arguments discussed briefly in §0.2 and Field's programme discussed in §1.2, the explanationist route to realism is not the only route to realism and, similarly, the representationalist route to nominalism is not the only route to nominalism. Nevertheless, given that these two routes to their respective views link up to interesting open questions about explanation and representation *simpliciter*, and given the popularity of these two routes in the recent literature, they will be the focus of the rest of this thesis.

In the next chapter I consider reasons given in the literature by realists for thinking that the route from mathematical explanations of non-mathematical facts need not go via a developed account of how these explanations function: that the presence of such explanations is sufficient in virtue of some other commitments that the nominalist opponent is taken to have. I argue that such reasons are unpersuasive.

Chapter 2

The enhanced indispensability argument and inferential conservativeness

In the previous chapter I argued that two conditionals are in need of assessing. One of these (the Explanation Conditional) is that *if* mathematics plays an explanatory role in science *then* our world-oriented uses of mathematics justify (some form of) mathematical realism. The enhanced indispensability argument, the focus of much attention in the recent literature, constitutes an inference from the existence of extra-mathematical explanations to the existence of mathematical objects. This chapter constitutes an argument for the claim that the enhanced indispensability argument cannot be assessed without an account of how mathematics plays this explanatory role. This claim may be quite fairly described as common-sense (*cf.* Saatsi 2016b, where the point is forcefully pressed that this is how the debate ought to proceed). However, the mathematical realist claims to offer reasons for thinking that their opponent is *already* committed to thinking that the inference from the existence of extra-mathematical explanations to mathematical realism is licit in virtue of some of their prior commitments. On this line, the realist's opponent's commitment to the inference being licit does not depend on any substantive claims about what extra-mathematical explanation is like, only on whether there are any such explanations. If this is right, then even though an investigation into extra-mathematical explanation might be interesting for the reasons set out in the introduction, it is *not* required for the assessment of the enhanced indispensability argument. It is worthwhile exploring this idea. This chapter consists in teasing these arguments out and arguing that they fail.

In section one I set out the enhanced indispensability argument, making explicit its salient features, and briefly discuss one example of a mathematical explanation of a physical fact.[26] The primary argument given in the literature for thinking that the mathematical realist's opponent is committed to holding that it is permissible to infer from the existence of extra-mathematical explanations to mathematical realism involves appealing to the purported fact that the enhanced indispensability argument is inferentially conservative over the arguments

[26] Further examples are set out in §2.7 as part of the set up for the assessment of accounts of extra-mathematical explanation in the next two chapters.

given for scientific realism. In short, the mathematical realist claims that the enhanced indispensability argument uses no inferential resources over and above those required to motivate scientific realism, a view that the mathematical realist's opponent is taken to endorse.

In section two I discuss this claim of inferential conservativeness, in section three argue that, depending on how it is understood, it is either undetermined by the available evidence or is false and in section four respond to objections. In section five I discuss and reject a second argument for thinking that the inference from the existence of extra-mathematical explanations to the existence of the mathematical objects (purportedly) appealed to in the explanation is licit: that, all other things being equal, there is an entitlement to a maximally general form of inference to the best explanation (IBE).

In section six I take stock and make the argument that, given that the previous two arguments fail, the argument can be progressed by developing an account of extra-mathematical explanation. In section seven I set out some case studies of mathematical explanations and in section eight I set out some criteria to be used in assessing accounts of extra-mathematical explanation. This sets up the discussion of rival accounts of extra-mathematical explanation, which takes place in chapters 3 and 4.

2.1. The enhanced indispensability argument

In this section I set out the enhanced indispensability argument for mathematical realism. Here is the argument in its canonical form, as introduced by Baker.

> (1) We ought rationally to believe in the existence of any entity that plays an indispensable explanatory role in our best scientific theories.
> (2) Mathematical objects play an indispensable explanatory role in science.
> (3) Hence, we ought rationally to believe in the existence of mathematical objects. (Baker 2009: 613)

As discussed in the previous chapter, the enhanced indispensability argument is offered in a dialectical context in which interlocutors seem to agree that mathematics playing just *any* indispensable role in mathematics is insufficient to motivate an adoption of mathematical realism. Rather, the argument goes, it is the fact that mathematics plays an *explanatory* role that we have reason to endorse mathematical realism. Although I will discuss some examples drawn from scientific practice in §2.7, the reasoning behind the enhanced indispensability argument can be seen with a simple example.[27] The best explanation of a parent's inability to evenly distribute their 23 strawberries amongst their 3 children involves the mathematical fact that 23 cannot be evenly divided by 3 – this mathematical fact, alongside some non-mathematical facts, explains the impossibility (Lange 2013a: 488). No non-mathematical explanations are as unifying, as simple and as general as this mathematical explanation: further, without the mathematical fact, we seem to have no explanation at all. Via IBE, the thought goes, it should be inferred that the mathematical objects that are the subject matter of the mathematical fact (23 and 3) exist and that they stand in the mathematical relation appealed to in the explanation. If 23 and 3 exist, then mathematical realism is true.[28]

In the literature, the truth of (2) is often equated with the existence of mathematical explanations of physical facts, like the strawberry explanation. These should be contrasted with non-mathematical explanations of non-mathematical facts (for example, explaining the start of the Russian Revolution by citing facts about the events that led up to it) and mathematical explanations of mathematical facts (for example, a proof that *explains why* a theorem obtains in addition to merely demonstrating *that* it does). It is taken to be the case that if there are extra-mathematical explanations of physical facts then mathematical *objects* play an explanatory role in science. As I will return to in chapter 4, this step is not so straightforward: once we have an account of extra-mathematical explanation in hand, it will become apparent that a piece of mathematics can give us information to explanatory information without it being the case that it is a *mathematical object* that is playing an explanatory role. What is important, for the moment, is that the mathematical realist takes it to be the case that (2) is confirmed by the existence of mathematical explanations of physical

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27 Given that the mathematical realist offers no reasons for restricting abductive inference to scientific contexts, it's not clear why the enhanced indispensability argument could not proceed just by appealing to non-scientific examples.

28 The existence of the mathematical objects appealed to in any given extra-mathematical explanation is enough to secure mathematical realism, if the view is understood as stating that there are at least *some* mathematical objects. The mathematical realist justifying their position using the enhanced indispensability argument has two routes from here. They can either argue that *just* the mathematical objects that appear in the best explanations of non-mathematical phenomena exist and that we lack justification for believing in any others: this would mirror the Quinean view that we are only justified in believing in the mathematical objects that are indispensable to our best theories, plus those required for rounding out (Quine 1986). The other route is to suggest that the enhanced indispensability argument can (indirectly) secure the existence of all mathematical objects. Such an argument might claim that the enhanced indispensability argument justifies belief in the objects present in extra-mathematical explanations, and then a separate argument can be given *from* the existence of these objects *to* the existence of the wider class of mathematical objects.

facts. I set out a range of examples towards the end of this chapter. The existence or otherwise of such explanations has no bearing on the arguments discussed in this chapter, which concern the first premise of the enhanced indispensability argument.

2.2. The inferential conservativeness argument

In this section I discuss what I'll call the inferential conservativeness argument. Let an argument A be inferentially conservative over another argument B just when A makes use of no inferential resources not made use of by B. A claim of inferential conservativeness is at the heart of the reasons mathematical realists give for thinking that their opponent ought to endorse (1). According to the inferential conservativeness argument, if the nominalist wishes to endorse arguments for scientific realism, they must endorse the first premise of the enhanced indispensability argument. This is because, the mathematical realist claims, the enhanced indispensability argument is inferentially conservative over the standard argument(s) for scientific realism. Below, I draw attention to places in the literature where mathematical realists present the inferential conservativeness argument. On closer inspection, the proposed inferential analogy is glossed far too quickly by the mathematical realist.

As the quotes in §2.2.1 will make apparent, the inferential conservativeness argument is almost always given in terms of what forms of inference the mathematical realist's opponent already accepts. This reference to the nominalist and the mathematical realist is, clearly, a stand-in for talking about a relationship between argument(s) for mathematical realism and scientific realism.[29] Nevertheless, it clarifies issues to talk about relationships between arguments rather than talking about what forms of inference this or that interlocutor judges to be admissible.

[29] So long as scientific realism is understood in a particular way (see §2.2.1) it will always come out as consistent to endorse scientific realism but not mathematical realism. What the mathematical realist means is that *reasoned* endorsement of scientific realism entails endorsement of mathematical realism, as the only good reasons for endorsing scientific realism are also good reasons for endorsing mathematical realism (so long as there are extra-mathematical explanations).

2.2.1. The inferential conservativeness argument in the literature

Contemporary mathematical realists take as their opponents those who wish to be realists about non-observable theoretical entities but non-realists about mathematical objects.[30] This seems a sensible target: it would be churlish to require from the mathematical realist an argument for their view that could be accepted by (for example) a constructive empiricist or someone who thinks that our scientific theories are in no sense a source of ontological commitment, especially given the indispensability argument's Quinean origins.[31] However, this fact about the dialectic is important. It is this fact that the realist takes to justify their strategy of not launching an independent defence of IBE in the extra-mathematical case, but instead only noting that the intended audience of their arguments *already* endorse IBE. Without offering an independent argument for the acceptability of IBE in the extra-mathematical case, a lot of weight is placed on the assumption that the nominalist enters the debate with a commitment to the inferential resources used by the mathematical realist. Accordingly, it is problematic for the mathematical realist if the inferential common ground is smaller than is presumed by the mathematical realist.

According to the inferential conservativeness argument, then, the mathematical realist's opponent has a pre-existing commitment to IBE being a valid form of inference which, when combined with the existence of extra-mathematical explanations, yields mathematical realism. The nominalist opponent is lumped with this inferential commitment, the thought goes, in virtue of their commitment to scientific realism. In one of the earliest contemporary discussions of the inferential resources appealed to by the scientific realist and mathematical realist, Colyvan says:

> [The indispensability argument] puts pressure on the marriage of scientific realism and nominalism […] because the style of argument is one which scientific realists already endorse. (Colyvan 2006: 227-8)

This proposed inferential analogy between the nominalist and the mathematical realist is also appealed to by Baker:

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[30] Therefore, for the rest of this chapter, the term 'nominalism' refers to the view that we are justified in believing in (at least some) unobservable physical entities but not in mathematical entities.

[31] Although, as it happens, it isn't so clear that the constructive empiricist can so easily be a mathematical nominalist (Leng 2010: 189).

> A crucial plank of the scientific realist position involves inference to the best explanation (IBE) to justifies (*sic*) the postulation in particular cases of unobservable theoretical entities. (Baker 2005: 225).

Making explicit the intended audience of his argument, he continues:

> Of course there are many philosophers who are not scientific realists, and alternative positions (notably constructive empiricism) are often based on a rejection of IBE in some or all cases. Nonetheless, the indispensability debate only gets off the ground if both sides take IBE seriously. (*ibid*)

For Baker and Colyvan, then, the perceived common ground between the mathematical realist and their opponent is that they both take IBE seriously. What does it mean to take IBE seriously? On one reading of 'take IBE seriously', it is sufficient for taking IBE seriously that one endorses it in at least some contexts. That is, if one takes IBE seriously, that it is treated as an open question, for a given explanation or a given kind of explanation, whether or not IBE is a permissible form of inference. An example of a position that fails to take IBE seriously, in this first sense, would one according to which all forms of ampliative inference are rationally unjustifiable (whether inductive or abductive), or the narrower view, held for example by van Fraassen, that abductive inference in particular is never rational. According to van Fraassen, endorsing IBE in any context violates Bayesian conditionalization (van Fraassen 1989). Compatibilists (Henderson 2013; Lipton 2004; Okasha 2000; Weisberg 2009) claim in response that the two can be reconciled.[32] So, an incompatibilist about IBE and Bayesianism like van Fraassen is an example of someone who Baker takes not to be in the intended audience of his argument. van Fraassen-style incompatibalism about abductive reasoning often goes hand-in-hand with non-realism about unobservable physical objects. I agree that the mathematical realist need not concern themselves with convincing such an incompatibabilist: the realist's goal is to place mathematical entities on a par with theoretical physical entities, which is a strange goal to have if one's opponent in the dialectic is a non-realist of some kind about such physical entities.

[32] There are various routes to making abductive inference and Bayesian conditionalization compatible. For the remainder of this thesis, I will simply assume that one of these routes is viable.

On the other hand, part of the above passage suggests that Baker has something more restrictive in mind: he seems to lump in with constructive empiricists those who reject IBE in "some […] cases" (Baker 2005: 225). If it is right that Baker is claiming that the debate ought only to be between those who already accept that IBE is a valid form of inference, no matter what kind of explanation is under consideration and other features of the context in which the explanation is being offered, then his restriction is far too strict. For example, someone who generally accepts IBE but rejects it in cases where there is only one explanation to hand, or rejects it in very contested domains like metaphysics, is someone who rejects IBE in some cases. There can be meaningful and interesting disagreement between those who agree that (a) permissible abductive reasoning, in at least some cases, grounds explanationist arguments for believing in a particular entity and (b) there are explanations that, on their face, look extra-mathematical.

Here is what I take to be the argument underpinning Baker and Colyvan's comments, and it is to the below argument that I shall refer to as 'the inferential conservativeness argument':[33]

1. Scientific realists ought to accept the form(s) of inference used in abductive arguments for scientific realism, under pains of their endorsement of scientific realism being unjustified.
2. Mathematical nominalists are scientific realists.
3. The enhanced indispensability argument is inferentially conservative over the abductive argument(s) for scientific realism.
4. If an agent judges the form(s) of inference used in argument A to be permissible, and argument B is inferentially conservative over A, then the agent ought to judge the form(s) of inference used in argument B to be permissible.
5. Scientific realists ought to accept as permissible the form(s) of inference used in the enhanced indispensability argument

[33] Busch & Morrison (Busch and Morrison 2016) have also recently understood the kind of reasoning offered by the realist along similar lines, appealing to the notion of parity between the mathematical and scientific realists. Although I find the notion of *arguments* being *inferentially conservative* more illuminating, nothing turns on this. As will be made clear in §2.3.2, the substance of my *response* to the realist line of reasoning is distinct from Busch and Morrison's, who aim to undermine parity/inferential conservativeness by making the claim that all the arguments for realism about unobservables are made in causal contexts whilst I explicitly ward *against* making this move. It's also worth briefly noting that I'm sceptical that transforming arguments into a deductive form is always helpful. Doing so can flatten the kinds of reasons that philosophers give in favour of views and the kind of reasoning that goes on in philosophical debate. (Worrall makes a similar point forcefully when discussing probabilistic renderings of arguments found in the scientific realism literature (Worrall ms: 21-27)). However, when what is at issue is a delicate dialectical point about what an opponent is or is not already committed to, drawing out the line of reasoning explicitly *is* helpful, so I have done so here.

The inferential conservativeness argument, then, is an argument from claims about a relationship between arguments to a claim about what particular agents ought to endorse or believe. Before assessing the argument, two issues need to be clarified. The first concerns the claim that the mathematical realist's opponent is a scientific realist and the second concerns which arguments are referred to by the term 'the abductive argument(s) for scientific realism'.

2.3. Against the inferential conservativeness argument

2.3.1. Are the mathematical realist's opponents scientific realists?

Many commentators writing on the relationship between scientific realism and the enhanced indispensability argument have taken it as read that the mathematical realist's nominalist opponent is, indeed, a scientific realist: the argument then proceeds by drawing analogies between the arguments given for scientific realism and the arguments given for mathematical realism. Most importantly, this claim is made by Baker and Colyvan, as indicated above. However, on many understandings of scientific realism, it *must* be false that the nominalist is a scientific realist. Leng notes that on a common understanding of scientific realism, the view *entails* mathematical realism. This is because our scientific theories "include amongst their laws assertions that imply the existence of mathematical objects" (Leng 2005b: 65).[34] This fact about our scientific theories means that any explication of scientific realism in terms of truth or approximate truth will be inconsistent with the nominalism: if our best scientific theories are true, or approximately true, then so too are the components that involve the existence of mathematical objects.[35] Assuming a face-value reading of existential statements, the approximate truth of our scientific theories entails the existence of mathematical objects.[36] This fact is, of course, the only way to make sense of the debate discussed in §1.5

[34] Saatsi also notes that "the Platonist could argue […] that the realist is committed to mathematical entities purely by virtue of [the truth/approximate truth] characterisation of realism" (Saatsi 2007: 23). I discuss Saatsi's commentary in more detail in the next section.

[35] Perhaps the view that our scientific theories are approximately true could be made compatible with nominalism if the claim was understood as saying that our theories' approximate truth means that the mathematical content is true *simpliciter* whilst the mathematical content is not. Such a route faces problems with mixed mathematical-physical content.

[36] The assumption of a face-value reading is a crucial one. Azzouni casts doubt on this face-value reading of existential locutions (Azzouni 2004; Azzouni 2010). If this programme can be made to work, meaning that the truth of sentences of the form 'there exists a P' does not entail that P *really* exists, then the nominalist can countenance both the approximate truth of our scientific theories and the non-existence of mathematical objects. Although the feasibility of an Azzouni-style response is of relevance to the nominalist's chances, it

about nominalistic adequacy. It is unsurprising, then, that it is easy to find many of the mathematical realist's nominalist opponents explicitly eschewing realism (at least when construed in terms of truth): for example, Leng states that she defends "an anti-realist view of science in rejecting the claim that we ought to believe that our best scientific theories are true or approximately true" (Leng 2010: 11).

That nominalists are not scientific realists (at least when this view is understood in terms of truth), and that the mathematical realists plausibly faultily ascribe this view (or at least do so without nuance), clearly does not mean that there is *nothing* to the inferential conservativeness claim. Rather, the argument should be clarified. Indeed, some other comments from Baker and Colyvan bring this out. Baker, for example, says that "a crucial plank of the scientific realist position involves inference to the best explanation (IBE) to justify the postulation *in particular cases* of unobservable theoretical entities" (Baker 2005: 225), and Colyvan takes his opponent to be someone who is "happy to go beyond what is unobservable and posit unobservable entities" (Colyvan 2006: 2). Whether or not the nominalist's opponent is someone who is properly described as a scientific realist, then, is both (a) dependent on one's understanding of scientific realism and (b) irrelevant to the plausibility of the inferential conservativeness argument. Baker and Colyvan may well take themselves to be working with a conception of scientific realism whereby one can count as a scientific realist by believing in particular unobservable theoretical entities, whilst Leng (at least when she eschews scientific realism on the basis of its truth entailing mathematical realism) is working with a conception that appeals to notions of truth.

What this brings out, however, is that what is at issue is whether or not the inferential resources standardly used to justify realism about particular unobservable theoretical entities are sufficient to also underpin the enhanced indispensability argument. This fact, combined with the fact that (at least some) nominalists explicitly disavow the label of 'scientific realist', means that continuing to appeal to this term is unhelpful. I will refer to 'realism about unobservables'[37] in places that others refer to 'scientific realism', to avoid this ambiguity and

seems orthogonal to whether or not the explanatory or representational uses of mathematics can justify ontological commitment. Indeed: *even if* Azzouni is right that the truth of seemingly-mathematically-committing sentences doesn't entail mathematical realism, it may still be the case that such a view is entailed by mathematics' explanatory or representational capacities, once fully analysed. These two routes to realism don't go straightforwardly through the truth of sentences that (on standard, non-Azzouni, readings!) existentially commit us to mathematical objects but rather through the roles that mathematical objects play in scientific theories.

[37] Where 'unobservables' refers to unobservable *physical* posits of scientific theories.

to avoid the awkwardness of ascribing to the mathematical realist's nominalist opponents a view that some of them eschew.[38] The fact that the mathematical realist's opponents cannot be universally construed as scientific realists plays a role in the next section.

2.3.2. The many arguments for scientific realism and realism about unobservables

Here is where we are. The inferential conservativeness argument is the main argument used by the mathematical realist to demonstrate that the nominalist ought to (or, rather, already *does*) endorse IBE in the extra-mathematical case, on the basis of their believing in the existence of particular unobservable objects on explanationist grounds. In this section I distinguish some different realist arguments and argue that the mathematical realist requires that the enhanced indispensability argument is inferentially conservative over a particular subset of these.

One pressing question when assessing the inferential conservativeness argument concerns *which* arguments for realism about unobservables should be considered: which arguments must the enhanced indispensability argument be conservative over? Much as, as demonstrated in the discussion above, there are various views that are fairly characterised as scientific realist views, there are various related arguments given for these views. There are two distinctions that might be useful in delineating the various arguments and views. To demonstrate that these distinctions are exhaustive requires more work than can be done here: but hopefully the distinction is sufficiently fine-grained for current purposes. The first distinction is between local and global arguments, sometimes referred to as a distinction between retail and wholesale arguments (Magnus & Callender 2004: 321). Local arguments concern *particular* scientific theories, and can reasonably be characterised as the inferences and arguments performed by scientists. Fitzpatrick says that such local arguments take as their motivation "the weight of the particular first-order evidence that led scientists to accept the claim in the first place" (Fitzpatrick 2013: 3) where an examples of such claims include "that there are atoms, that past and present organisms on earth are the product of evolution by natural selection, that the continents move laterally on tectonic plates" (*ibid*). Local arguments contrast with global arguments. Global arguments take as their conclusion, rather

[38] Another virtue is that whilst the umbrella term 'scientific realism' may well contain views like (ontic or epistemic) structural realism, 'realism about unobservables' does not. It is unclear whether or not the inferential conservativeness claim has any purchase *at all* on the ontic or epistemic structural realist: these are clearly not the kinds of realists that Baker and Colyvan take themselves to be targeting.

than any particular first-order scientific claim, a claim about *scientific theories*: that they (or, at least, a subset of them) are (approximately) true, for example. Claims like this contrast with rival claims about our scientific theories made by the realist's opponent (for example, that they are merely empirically adequate). The no-miracles argument is standardly taken to be a global argument of this kind: it argues from one (purported) fact about our best scientific theories (that they generate novel successful predictions) to another (purported) fact about our best scientific theories (that they are approximately true), on the basis that the latter fact is the best explanation of the former fact.[39]

A second natural distinction is between realist views that deal in the notion of truth and those that do not. The former include views, discussed above, that claim that our best scientific theories are (approximately) true. The latter are views that deal only in notions of existence: a view according to which atoms exist is a realist view (in the sense that it goes beyond what, for example, a constructive empiricist is willing to say) yet it does not go as far as to make the claim that our best scientific theories are true.[40] Many so-called 'selective realisms' belong to this latter category. As discussed in the previous section it seems as if, whatever sense in which the mathematical realist's opponent is a realist, it cannot be in the sense that they hold that our best scientific theories are (approximately) true.

With these distinctions in mind, it should be asked which of these arguments the enhanced indispensability argument ought to be inferentially conservative over: would it be sufficient, for example, if the enhanced indispensability argument turned out to be conservative only over wholesale arguments, or only over arguments that had as their conclusion a claim about the properties of our scientific theories rather than about the objects that are the subject matter of these theories?

Saatsi suggests that, in considering parallels between the use of IBE by the scientific realist and the mathematical realist, we ought to focus on the no-miracles argument, which is standardly understood as a global argument, with the conclusion a claim about our scientific theories: that they are (approximately true). He says we ought to do so because:

[39] I won't engage in evaluation of whether or not any of these routes are successful. It is also important to stress that the boundary between local and global arguments is unlikely to be sharp.
[40] Assuming a face-value reading of existential locutions, a realist view that deals in the notion of truth strictly says more than the view that deals only in existence.

> [I]t makes the strongest, overarching use of IBE and all the standard super-empirical virtues. Furthermore, this argument does not only defend the reliability of the scientists' inferences, but of inference to the best explanation more generally, as the No Miracles argument is justified by virtue of being itself a philosophical inference of the same form. (Saatsi 2007: 26)

In claiming that the argument that ought to be considered is that which goes further than defending "the reliability of scientists' inferences" (*ibid*), it seems clear that Saatsi has in mind a global argument. There are two reasons why focusing on the global no-miracles argument isn't appropriate.[41] The first is that focusing on just this one argument is too narrow in scope. If Baker's claim is (something like) "being a realist about unobservables on explanationist grounds requires endorsing a form of IBE that applies also in the extra-mathematical case", then it doesn't suffice that there is *some* explanationist argument for realism about unobservables that uses IBE in such a way. Baker requires something stronger. It must be the case that the enhanced indispensability argument is inferentially conservative over *all* of the good arguments for realism about unobservables.

I take it, though, that the purpose in focusing on the global no-miracles argument is supposed to be that if the analogy does not hold up *even in* the strongest possible use of by the realist about unobservables, then it is safe to infer that the analogy will not hold up in the cases where a less powerful form of abductive reasoning is used.[42] The second reason, then, that we should not inspect the no-miracles argument is that the mathematical realist's opponent cannot endorse the no-miracles argument, understood as a global argument for the (approximate) truth of our best scientific theories. This isn't because endorsing this argument leads to endorsing the enhanced indispensability argument, via inferential conservativeness: rather, endorsing the no-miracles argument *suffices* for the truth of mathematical realism for the reasons discussed above.

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[41] It is important to note that Saatsi argues that *even if* we focus on the no-miracles argument, taking as its conclusion a claim about scientific theories rather than any particular unobservable posit, the proposed analogy between the inferential resources at use here and in the enhanced indispensability argument *still* does not hold up. Given that there is no need to defend the consistency of endorsing the global no-miracles argument but not endorsing the enhanced indispensability argument, I do not engage with this aspect of Saatsi's argument. If Saatsi is correct that (in my terms) the enhanced indispensability argument is not inferentially conservative over the global no-miracles argument, and I am correct (as argued below) that the realist has not demonstrated that it is inferentially conservative over the local arguments, then the case against the inferential conservativeness argument is all the stronger.

[42] Given that one type of an argument takes as its conclusion a claim about unobservable objects and the other about properties had by scientific theories, it is not clear why we should think this.

Recall that in setting up the comparison, Baker says that the "scientific realist position involves inference to the best explanation (IBE) to justifies (*sic*) the postulation in particular cases of unobservable theoretical entities" (Baker 2005: 225). It is this use of IBE, then, that Baker has in mind. The global no-miracles argument is decidedly *not* a use of IBE to justify the postulation of unobservable theoretical entities in *particular cases*: it is the use of to support a much more general claim, about our scientific theories. This mirrors, also, the use of IBE by the mathematical realist. Mathematical realists typically do not proceed by claiming that the existence of some extra-mathematical explanations justifies belief in mathematical objects as a class of objects, or that our mathematical claims *in general* are true: rather, they make the narrower claim that can be used to justify belief in the mathematical entities appealed to in the particular extra-mathematical explanations (the truthmakers of the number theoretic theorem appealed to in the cicada explanation, for example).[43]

It is clear in what Saatsi says in the above passage that he has in mind a global form of the no-miracles argument. Recall that a local no-miracles argument is that being made when reasoning from some *particular* best explanation of an observable fact to the truth of the best explanation and to the existence of the objects referred to in that explanation. In contrast, a global no-miracles argument appeals to (something like) a theoretical or philosophical explanation of a fact about our scientific theories as a whole: that they achieve novel predictive success. This is very much *not* the kind of explanation at play in cases of local abductive inference. In a given case of local abductive inference, the explanation in question will often be causal, but it is clear that whatever form of explanation is in question when the explanandum is 'our scientific theories achieve novel predictive success', it is not causal: the (approximate) truth of our scientific theories does not *cause* them to generate novel predictive successes. By requiring that we focus on the global no-miracles argument, Saatsi sets the bar too high. One can perfectly consistently be a realist about unobservables, motivated by a conjunction of local arguments, yet *reject* the permissibility of abductive reasoning outside of these narrow scientific contexts: i.e., in contexts in which the explanandum is a philosophical position like scientific realism or mathematical realism. So, the question must be: in providing arguments, that go via explanation, for realism about particular unobservable entities, do realists about unobservable entities make use of a form of inference that makes the enhanced indispensability argument inferentially conservative over these arguments?

[43] See fn.28 for discussion of the relationship between realism about the particular mathematical objects present in a given extra-mathematical explanation and realism about the wider class of mathematical objects.

If the mathematical realist's opponent is a scientific realist, then they must be of the kind that endorses the existence of particular unobservable entities, rather than of the kind that endorses the approximate truth of our scientific theories. Therefore, the arguments that they can reasonably be expected to give for their views about unobservable physical entities are non-global, local arguments, found in particular scientific contexts. If this is the case, then it is false that they endorse anything like the maximally general form of IBE that is appealed to in the first premise of the enhanced indispensability argument. The inferences that justify belief in particular unobservable physical objects are likely to be piecemeal and not susceptible to a general description.

The following comment from Magnus and Callender is instructive:

> We acknowledge that it may be possible to get a kind of wholesale argument by discovering something in common among all good retail arguments for realism. Without trying to settle the larger epistemological issue, we offer a note of caution. Reflecting on the vast complexities of various historical episodes in science, there is no reason to think that the general assumptions one finds will be at all simple, natural or even non-disjunctive; in short, there is no guarantee that the criterion one finds will be either interesting or useful. So although it is logically possible to turn a retail argument into a kind of wholesale argument, the resulting wholesale argument may appeal to 'general assumptions' that are long, gruesome and can do none of the heavy lifting that wholesale arguments are usually meant to do. (Magnus & Callender 2004: 335)

There is no need to share in Magnus and Callender's pessimism about uniting local arguments to think that the surface level disunity of local realist arguments puts the ball in the mathematical realist's court. In order to demonstrate that the enhanced indispensability argument is inferentially conservative over these local arguments for realism about particular entities, the mathematical realist must either demonstrate that *for each of these arguments*, the enhanced indispensability argument is inferentially conservative over that particular argument or they must identify some common core or structure of all of the local arguments and argue that the enhanced indispensability argument is inferentially conservative over it. The mathematical realist, it is clear, has not done this. It may well be that there is something that unites local arguments, that they all share inferential resources *and* that these inferential resources just are those appealed to in the enhanced indispensability argument – but this is yet to be demonstrated.

It is important to distinguish the response given here to another, similar, response that might be given. One alternate route to using the fact that the mathematical realist's opponent must make use of local arguments, rather than anything like the no-miracles argument, to dislodge the inferential conservativeness claim would be to attempt to isolate something that is core to all of the local inferences and then argue that the inferential conservativeness claim is false relative to this core of all local arguments. One such attempt might be to argue that all of the local arguments for realism about unobservables appeal to only in causal contexts (Busch and Morrison are proponents of this kind of move (Busch & Morrison 2016)).[44] There are a few reasons why this route isn't a good idea. The first is a worry about question-begging: as Baker notes, "the 'no noncausal explanation' thesis is not one to which the nominalist can appeal without begging some pivotal questions" (Baker 2005: 229).[45] The second are concerns about the truth of the claim that arguments for realism about particular observables are always deployed only in causal contexts (Colyvan 2001: 45) and the truth of the claim that extra-mathematical explanations are not causal. The first worry, here, is that in making this claim about the kinds of explanations at play in local cases (the claim that they are always causal) threatens to result in another rallying back and forth of cases, each to be defused one by one – the realist provides a local case that looks non-causal, the nominalist replies either that the context is in fact causal or, in fact, this is not a context in which IBE is licit, and so on. The second worry is that, as I discuss in miniature in §4.1 and throughout the rest of chapter 4, the claim that extra-mathematical explanations are not causal is, at the very least, not *quite* as straight forward as this response would have it and so it is unwise to make a crucial move dependent on this delicate question. The third, and perhaps most pressing,

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[44] See, also, perhaps, Field's preference for intrinsic over extrinsic explanations (Field 1980: 44).

[45] Baker's point here is that ruling out IBE in non-causal cases begs the question against the realist exactly because mathematical objects are non-causal. Perhaps, though, for Baker to press the question-begging charge is to misunderstand the dialectic. The mathematical realist says: the inferential resources needed to secure mathematical realism just are those that the scientific realist appeals to in their explanationist arguments for their view. The Busch & Morrison-style response is: many plausible explanationist arguments for realism about unobservables require amongst their inferential resources only the permissibility of IBE in causal cases, not in all cases and, therefore, the enhanced indispensability argument is *not* inferentially conservative over arguments for realism about unobservables, because extra-mathematical explanation is noncausal. To point this fact out about the inferential resources of various explanationist arguments is *not* to make the much stronger claim that IBE is, *in fact*, only permissible in causal cases. The claim that explanationist arguments for realism about unobservables only require endorsement of IBE in causal cases is consistent both with IBE being applicable in extra-mathematical cases and IBE not being applicable in extra-mathematical cases.

though is that this route simply does work that is not dialectically required. Once it is pointed out that, under pains of not being automatically committed to mathematical realism, the mathematical realist's opponent cannot support their realism about unobservables by appealing to the no-miracles argument and instead appeal to a diverse set of local arguments, the burden is on the mathematical realist to argue that the enhanced indispensability argument is inferentially conservative over these, or over some common core that they all share. There is no need to take the further epistemically risky step of identifying some common core to all of these local arguments and then arguing that the enhanced indispensability argument fails to be inferentially conservative over them.

2.4. Objections

Here is a first worry about the response to the inferential conservativeness argument offered above. It is possible that the realist might respond to the above argument against the conservativeness claim by gesturing towards arguments for realism about unobservables that do, in fact, seem to rely on a form of abductive reasoning that *is* analogous to that used in the enhanced indispensability argument. A global argument that had as its conclusion that the objects that feature in our best scientific theories exist, for example, would be such a theory: it is also one that the mathematical realist's opponent *could* accept to motivate their realism about unobservables (given that it does not appeal to notions of truth). According to this argument, the existence of unobservable entities is offered as the best explanation for the success of science.

However, as I said above when discussing Saatsi's response, it will not do to establish the realist's conservativeness claim that there exists an explanationist argument for realism about unobservables that appeals to the liberal form of IBE that the mathematical realist requires. Instead, if Baker takes his target to be the nominalist who is also a realist about unobservables, something stronger must be the case. Remember, the claim to be argued for is that if the mathematical realist's opponent wants their realism about observables to be rationally justified, doing so requires using a form of abductive inference that also applies in the extra-mathematical case. It must be the case that the only route to realism about unobservables is via this sort of global argument that (plausibly) uses the same inferential resources as the enhanced indispensability argument. To the contrary, in the scientific realism literature it is treated as a live option that there is a plausible local route to realism about unobservable physical entities that, accordingly, does not appeal to the form of abductive reasoning that features in the first premise of the enhanced indispensability argument.

This brings to bear, however, a second worry. It is important to note that the argument in this section is *not* the following: IBE, it turns out, is an admissible form of inference only in the particular local contexts that feature in arguments for realism about unobservable entities and, in virtue of extra-mathematical explanation not being such a context, IBE is not an admissible form of inference in the case of extra-mathematical explanation. As noted above, the mathematical realist is likely to maintain that arguments of this kind are question-begging, just as Baker considers the claim that IBE is permissible only in causal contexts to be question begging. Indeed, the inference from the fact that a set of inferential resources are used in local arguments for realism about particular unobservables to the claim that these are the *only* permissible forms of inference seems suspect. However, that is not the claim being made here. Rather, the claim is merely that the mathematical realist's nominalist opponent is wise to support their realism about unobservables by appealing to a series of local arguments and, as such, they have no commitment to an overarching NMA-style endorsement of IBE. It may well be that the enhanced indispensability argument is inferentially conservative over all of these local arguments (and, as such, the nominalist cannot reject the enhanced indispensability argument without being inconsistent[46]) but the mathematical realist has not demonstrated this. Gesturing towards a general endorsement of abductive reasoning is unlikely to be a descriptively adequate account of the inferential resources used in the series of local arguments.

2.5. The argument from abductive maximalism

So, it seems false that in order to be a realist about unobservable physical entities on explanationist grounds, one must endorse a form of argument that the enhanced indispensability argument is inferentially conservative over.[47] Whether or not any of these routes to realism about unobservables that appeal to weaker inferential resources are successful is beyond the current scope: but what has been demonstrated is that the mathematical realist's analogy between arguments for realism about unobservables and

[46] Assuming that they accept that there are extra-mathematical explanations.
[47] The caveat that what is relevant is the possibility of being a realist *on explanationist grounds* is important: Baker's claim is not that it is *impossible* to both be a scientific realist and a mathematical nominalist (as these views are clearly consistent), but rather that the only good arguments for the former view entail that the enhanced indispensability argument is a good argument.

mathematical realism are on shaky ground. In this section I discuss a second route that the realist might take to the conclusion that their opponent already does (or should) endorse a form of abductive reasoning sufficient to underpin the enhanced indispensability argument.

This second way for the realist to move from the arguments for realism about unobservables to the enhanced indispensability argument is to argue that *if* one endorses abductive reasoning in one context *then* (absent good defeaters), this entails a commitment to a maximally general form of IBE. On this line of reasoning, even if the inferential resources used in arguing for realism about unobservables only involve abductive reasoning in restricted contexts, the reason that such reasoning is licit in this context is *because* IBE is generally permissible. In short, the realist may well claim that there is a default presumption in favour of a maximally general form of IBE, so long as one is not the kind of global sceptic discussed briefly in §2.2.1. Similarly, they might argue that the best explanation of the fact that abductive reasoning is licit in some cases is the truth of some more general claim about a link between explanation, ontology and truth. (I'll just assume for the sake of argument that this last claim is not problematically circular).

The abductive-maximalist's claim can, of course, only be that there is a *defeasible* presumption of the permissibility of maximally general IBE. There are (at least!) two routes to defeating this presumption. The first is to gesture towards the diversity of kinds of explanation that can be given. Explanations can be given in a range of scientific contexts and these explanations, on the surface at least, seem to be very diverse. Moreover, there are explanations that can be given outside of scientific contexts: there are plausibly mathematical explanations within mathematics (Kitcher 1984; Mancosu 2001; Tappenden 2005) folk psychological explanations (Bennett 1991), moral explanations (Majors 2006) and normative explanations (Väyrynen 2013).[48] Given the diversity of explanations and explanatory contexts, it may well be the responsibility of an abductive-maximalist to give an account of what it is that is present across all of the things that we call explanations that makes it such that, when we have the best explanation of some phenomenon, it is permissible to infer to

[48] Of course, the existence of such explanations might not always be used as a premise in an abductive argument. The existence of an explanatory proof of a theorem is not used as a premise in an argument that the theorem holds. (However, Maddy's notion of mathematical depth and the role that it plays in axiom choice has many parallels with appeals to the explanatory virtues in theory choice in science (Maddy 2011) so it is not entirely clear that explanation plays no justificatory role in mathematics).

the truth of that explanation and to the existence of the objects appealed to in the explanation (no matter what the explanation is).

There are two *prima facie* reasons not to place too much faith on this response that draws upon the diversity of explanations. The first is that it rests on a delicate burden of proof question and I do not know whether there are interesting and decisive ways of resolving burden of proof questions. Does the burden of proof lie with the abductive maximalist to provide an account of what it is that all explanations have in common that makes IBE permissible, no matter what the domain? Or does the burden of proof lie with the opponent of abductive maximalism to argue what makes extra-mathematical explanation sufficiently different from the uncontroversial cases such that it is not safe to assume that IBE is licit in the former case? The second is that, as briefly discussed in the introduction, there is a burgeoning recent debate as to whether or not there *are*, in fact, explanations of various different kinds.[49] Pursuing this response, then, relies on there, in fact, being various kinds of explanation. It is undesirable for this question to rest delicately on the outcome of these (reasonably orthogonal) investigations.

Here is a more straightforward way of arguing against abductive maximalism. Abductive maximalism is false if there is a case of an explanation being the best explanation of a given explanandum yet it being illicit to infer to the truth of the explanation.[50] And, indeed, there are such cases and they are cases that ought to be familiar from the literature on the Quine-Putnam indispensability argument. Recall, in the previous chapter, the discussion of the motivations for moving from the Quine-Putnam indispensability argument to the enhanced indispensability argument. One of the motivations, cited by Baker in the paper in which he introduces the canonical form of the enhanced argument (Baker 2005) is that the Quine-Putnam argument is usually understood as relying on confirmational holism and there are reasons to be suspicious of the holist presumption.[51] Doubt is cast on confirmational holism by cases in which a theory appears to be accepted as the best theory of some domain, but

[49] Reutlinger argues that the counterfactual account of explanation can accommodate even metaphysical explanation (Reutlinger 2017b), a form of explanation that seems (on the surface) most distinct from regular causal explanation.
[50] The sceptic about inference to the best explanation will argue that all situations are like this: in as much as the best explanation we have to hand is the best *of the explanations that we have conceived of*, rather than the best of all the logically possible explanations (van Frassen 1980: 143) This kind of case will not be of use here, given that all of those engaging in the debate must admit that inference to the best explanation is licit in *at least some* contexts.
[51] As in the previous chapter, I will bracket the debate as to whether or not confirmational holism *is* in fact required for the argument to go through: see Busch 2011 for discussion.

belief in the entities that the theory apparently refers to is withheld. It is less frequently noted, however, that these case studies also cast doubt on a general link between explanation and ontological commitment (what I have been calling 'abductive maximalism').[52]

Recall from §1.3 that historical episodes are marshalled to cast doubt on confirmational holism.[53] Maddy's examples and their role in undermining the Quine-Putnam indispensability argument have received much discussion in the literature (even if this has subsided along with the increased prominence of the enhanced version of the argument): here I only want to stress the role that *explanation* plays in the atom case. The role of this historical episode is usually used to cast doubt on confirmational holism (or, at least, on its consistency with a broadly Quinean approach). However, if it is the case (as Maddy claims) that the atom was the fundamental unit of chemistry in the period 1860-1913 (during which scientists withheld belief), then this surely entails that the atom appeared in the best explanations of many observable phenomena during this period.[54] Indeed, Maddy explicitly refers to "the explanatory power or the fruitfulness or the systematic advantages of thinking in terms of atoms" (Maddy 1992: 281) and notes that belief was withheld in the face of this. Maddy goes on to suggest that only once "directly verifiable" (*ibid*: 281) were atoms believed in. Both as a descriptive, historical claim and as a piece of ontological advice, this surely has its potential problems (see Colyvan 2001: 91-92, 98-105 for discussion). However, one need not endorse these further claims to agree that *if* the atom case study tells against confirmational holism (as the realist interlocutors seem willing to agree) then it *also* tells against abductive maximalism: the view that, no matter the context or the kind of explanation at hand, an object appearing in the best explanation of some observed phenomena justifies belief in that object.

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52 Despite mentioning these cases in the same breath as introducing the enhanced argument, Baker does not seem to realise that the cases *also* cast doubt on one of the premises of the enhanced argument. An exception is Pincock, who appeals to Maddy's discussion to motivate a restriction on permissible forms of inference to the best explanation (Pincock 2012: 215-17). However, the role that the cases play in the dialectic for Pincock is different from that which they play here: as noted, Pincock appeals to it in order to abstract away from it some general principles uses to place restrictions on permissible uses of inference to the best explanation (discussed below), whilst here it is used as a counterexample to abductive maximalism.

53 As in §1.3 and as with Pincock's discussion, this draws on Maddy's discussions of the case (Maddy 1992; Maddy 1997).

54 Maddy also discusses cases where idealized, false assumptions seem to play a crucial role in our best theories: "the analysis of water waves [assumes] the water to be infinitely deep", for example (Maddy 1992: 281). However, to appeal to these cases to argue against abductive maximalism requires defending the claim that idealizations can play an indispensable *explanatory* role.

There is one tension with using the case study to undermine abductive maximalism in a way that is precisely analogous with Maddy's use of the case to undermine confirmational holism. As Maddy notes, and is noted by commentators when discussing her objection (Busch 2011; Colyvan 2001; Leng 2010), the way in which the examples cast doubt on holism is by to demonstrating that there is a tension between the premise of the indispensability argument appealing to naturalism and the premise appealing to confirmational holism.[55] [56] However, in the enhanced indispensability argument, there is no naturalistic premise for abductive maximalism (underpinning the first premise) to be in tension with. A proponent of a maximally general form of IBE, then, could well put their foot down, as it were, and claim that a maximally general form of IBE is permissible and that the scientists described in Maddy's historical case study are simply mistaken.

However, if the proponent of abductive maximalism is going to take the route of maintaining that the scientists in question are mistaken, then they ought to provide an argument in favour of the view. Without such an argument, it is a natural presumption to take scientific practice as a guide. As has been stressed in this chapter, those arguing from the existence of extra-mathematical explanation to mathematical realism offer little explanation as to why we ought to think that all the things that we call explanations are such that they can generate ontological commitment.

2.6. Moving forward

What the above reveals is that trying to eke out a commitment to a form of abductive reasoning that can do the work in the extra-mathematical case, just using the resources of arguments for the existence of unobservables that the nominalist may or may not be committed to, is untenable. The range of various views and various arguments in this vicinity is too diverse for the mathematical realist to hit their target. Establishing that the enhanced

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[55] Some presentations (Colyvan 2001: 11) do not separate out the naturalism premise and the confirmational holism premise. Presentations that do so (Leng 2010: 7) are more helpful for exactly the reasons that become apparent when discussing Maddy's objection.

[56] There is ambiguity as to exactly what naturalistic premise must be accepted for the counterexamples to have their intended effect. Recall that, rather than confirmational holism being in tension with the naturalism premise of the indispensability argument as standardly understood (as entailing commitment to only the entities appealed to by our best scientific theories), the counterexamples show that confirmational holism is in tension with (something like) a broadly naturalistic spirit. An endorsement of a broadly naturalistic spirit, however, is decidedly *not* what the naturalism premise of the indispensability argument states. I won't pursue this point here. Colyvan argues that the atom case can be made consistent with a broadly naturalistic spirit, so long as we have a nuanced understanding of what the latter requires (Colyvan 2001: 98-105).

indispensability argument is inferentially conservative over *every* argument that might justify realism about unobservables is a tall order and not one that mathematical realists have begun to undertake. Furthermore, establishing that the enhanced argument is inferentially conservative over *a particular* argument that might justify some form of realism (as may be the case for the global no-miracles argument[57]) is not enough to lump a realist about unobservables with a form of abductive reasoning that can be used in the extra-mathematical case. There is also, I have argued, no good reason to endorse a maximally general form of IBE and that there appear to be historical case studies that tell against it.

A nominalist may choose to stop here and argue that the mathematical realist has not done the work necessary to demonstrate that the nominalist is committed to the right kind of abductive inference. This, however, would amount to winning on a burden-of-proof technicality. The realist can quite reasonably press that (at least some of) the purported cases of extra-mathematical explanation *sure look like* the kind of local cases in which the nominalist is willing to entertain commitment to the entities involved. At best, the realist continues, it should be treated as an open question as to whether these cases function in the sort of way that licenses ontological commitment. I think this is right. Even if, as I have argued, the realist arguments (that their opponents are *already* committed to thinking that the cases of mathematical explanation are the right kind to license ontological commitments) fail, the next step for both parties should be to inspect such cases in more detail.

There are two sorts of route to addressing whether or not IBE is permissible in the extra-mathematical cases. In this section I set these two options offer some considerations in favour of taking the route that I do in the rest of the thesis.

The first route is to try and abstract formal restraints from historical case studies. Suggestions of this form will isolate properties of a given explanation that make it such that IBE is permissible or impermissible: attempts to provide some kind of rule as to when inference is licensed in local cases. Pincock's suggested restriction is of this form.[58] In this short section I discuss Pincock's restriction as an example of this strategy.

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57 Although, as before, see Saatsi's scepticism (Saatsi 2007).

58 So, too, is Busch's requirement that the mathematics that appears in the best explanations of non-mathematical facts is consistent across theory change (Busch 2011).

Pincock uses historical case studies (like the case that Maddy appeals to concerning the history of the atom, discussed in §2.5) to motivate the following formal restriction on the permissibility of IBE:

> **Sensitivity**: a claim that appears in an explanation can receive support via inference to the best explanation only when the explanatory contribution tells against some relevant alternative epistemic possibilities. (Pincock 2012: 214)

There are two things wrong with this formal requirement. The first is that it does not appear that it can do the work that Pincock requires of it – that is, it cannot rule out IBE in the extra-mathematical cases.[59] However, there is a second, more relevant problem. If we think that there is a diverse range of kinds of explanation, then there is no guarantee that a formal requirement that a causal mechanical explanation (for example) must meet for IBE to be permissible will also be a requirement than an explanation of a different kind must meet. That is, even if *Sensitivity* is a formal requirement on the permissibility of IBE for the kind of explanation that the atom explanation belongs to, the mathematical realist may well press that there is no good reason to think that it is also a formal requirement on the permissibility of IBE in the extra-mathematical case.

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[59] Here is why. Pincock uses a variation on *Cicadas* to argue that extra-mathematical explanations will not meet Sensitivity. The variation is identical to *Cicadas* save for the fact that the number-theoretic theorem is replaced with the claim that "prime periods of *less than 100 years* minimize intersection (as compared to nonprime periods)" (Pincock 2012: 212) (emphasis in original). Pincock does not offer a principled way of working out, in any given case, what the relevant epistemic possibilities are. The invocation of the notion of relevance makes working out what the epistemic possibilities are more difficult still: it is natural to think that relevance is determined by contextual factors. If the epistemic possibilities are to be indexed to particular users of an explanation, then it seems that there could be some user for which 'there are no natural numbers' is an epistemic possibility. If so, then it seems like, for *this* user in *this* context at least, *Cicadas* does indeed tell against (at least one of) the relevant epistemic possibilities even if it does not tell against some alternatives concerning how many natural numbers there are. It would be a peculiar result that IBE is permissible for this agent, given their background beliefs, but not for another agent, even when faced with the exact same explanation. The second worry concerns whether or not *Sensitivity* is different from the requirement that the mathematical fact is indispensable, in addition to being explanatory. If it is the case that there are two equally good explanations (a claim Pincock endorses (*ibid*: 216)) with different number-theoretic theorems, then this seems like a demonstration that neither of these number-theoretic theorems are indispensable to the explanation. This worry stems from an ambiguity in Baker's formulization of the EIA. One way of reading the premise concerning explanatory mathematics is as saying that there is some particular mathematical fact that features in the best explanation (or all of the best explanations) of some particular explanandum, whilst another reading is that there is some particular explanandum such that all of the best explanations of that explanandum feature some mathematical fact (where the mathematical fact need not be common across all of the explanations of the explanandum). It is not immediately clear which of these requirements the mathematical realist should be required to meet. Given the possibility of constructing any mathematical claim using set theory, meeting the stronger requirement will be difficult.

This suggests a second route: getting clearer on how extra-mathematical explanation functions and using this to investigate whether, for each example of extra-mathematical explanation appealed to by the mathematical realist, an inference to the existence of the relevant mathematical object is justified. Hopefully the contrast with Pincock's methodology is clear. Whether or not an explanation satisfies *Sensitivity*, for Pincock, doesn't turn on any further details of what kind the explanation is or on how the explanans plays a particular explanatory role: it is met (or not) depending on the relationship between the explanation and the agent's epistemic possibilities. This route carried out in the next two chapters, in contrast, involves getting clearer on how the paradigm cases of extra-mathematical explanation function, by constructing an account of extra-mathematical explanation.

2.7. Examples of extra-mathematical explanation

In this section I set out four explanations that make use of mathematics: *Cicadas, Levy walks, Bridges,* and *Pendulum*. I make explicit the features that are salient for the discussion of accounts of extra-mathematical explanation in the next two chapters.

The first explanation is that which has dominated discussion of extra-mathematical explanation in the recent literature.

Cicadas

Cicadas spend much of their lives under the ground, and then emerge from the soil for a short period to mate and die. A striking fact about the life-cycle lengths of some particular North American cicada species is that they are prime: either 13 or 17 years, depending on the particular species. Biologists have offered an explanation of this striking fact, one that Baker formalises as follows:

(1) Having a life-cycle period that minimizes intersection with other (nearby/lower) periods is evolutionarily advantageous.
(2) Prime periods minimize intersection (compared to non-prime periods).
(3) Hence, organisms with periodic life cycles are likely to evolve periods that are prime. (Baker 2005: 233)

> Combined with information about ecological constraints, the explanation yields specific predictions about cicada life cycles (or, alternately, explanations of more specific explananda):
>
> (4) Cicadas in ecosystem-type E are limited by biological constraints to periods from 14 to 18 years.
> (5) Hence, cicadas in ecosystem-type E are likely to evolve 17-year periods. (*ibid*)
>
> Biologists suggest that in the evolutionary history of the cicadas, the cicadas shared their environment with creature that both also had periodical life cycles and that it would have been evolutionarily advantageous for the cicada to avoid – such as predators. This is the justification for (4) – by minimizing the intersection of their periods with others, the cicadas minimize the extent to which they interact with their periodic predators.

The first salient feature of *Cicadas*: it is ambiguous as to whether or not it is dialectically permissible to appeal to it when mounting the enhanced indispensability argument. The explanandum of the more general form of *Cicadas* appears to have mathematical content: on a face value reading, it appeals to a mathematical feature of the life-cycles of cicadas, their primeness (Bangu 2008). As such, it seems as though appealing to *Cicadas* is illegitimate in the same way as appealing to mathematical explanations of *mathematical* facts: in that it is question-begging (Leng 2005a: 174). The second salient feature is that *Cicadas* is an optimality explanation. That is, it explains the presence of some trait or behaviour by appealing to the fact that it is optimal. In this case, having prime periods is the optimal strategy for avoiding periodical predators.[60] There is reason to think that similar mathematical optimality explanations can be found throughout the scientific literature (Baron 2014).[61] That mathematical optimality explanations are likely to be widespread strengthens the demand for a general account of the functioning of such explanations, rather than piecemeal analyses of particular cases. In a recent discussion, Lange notes a worry that the standard explananda of presentations of *Cicadas* are not those that have an extra-mathematical (or what he calls

[60] When discussing optimality more generally, it's important to note that there may well be discrepancies between the observed behaviour and the behaviour that the theory indicates is optimal and therefore predicts that the organisms will exhibit. This can be bracketed here, however.

[61] Chirimuuta suggests that efficient coding explanations in cognitive neuroscience are extra-mathematical explanations in the same way that optimality explanations from biology are (Chirimuuta forthcoming). I return to Chirimuuta's discussion in chapter 4.

'distinctively mathematical') explanation. I will leave the explanations in the paradigmatic form in this chapter – but see fn.87 for a response to Lange on this point.

Baron provides one other example of a mathematical optimality explanation, which I am calling *Levy walks,* involving the predation patterns of sharks (Baron 2014), set out below. The purpose is to support the claim that explanations like *Cicadas* will be widespread.

> *Levy walks*
>
> Some animals, when they are foraging in environments with relatively few prey, move in a pattern described as a Lévy walk: "a random sequence of larger jumps, interspersed with several smaller jumps with frequent reorientation" (Baron 2014: 476). Why do the animals move in this way? Mathematics is used to demonstrate that moving in accordance with a Lévy walk maximises the chances of coming across targets, given that targets are distributed randomly and relatively scarcely. This fact, combined with facts about it being adaptively beneficial for the animals to move in a way that maximises their chances of finding prey and information about accordant trade-offs, explains why it is that some animals move in accordance with a Lévy walk. (*ibid*)

Here is the third case, concerning the bridges of Königsberg, introduced into the literature by Pincock (Pincock 2004).

> *Bridges*
>
> Everyone who attempted to walk across the seven bridges of Königsberg, passing over each bridge exactly once, failed to do so. Why? The islands and bridges constitute a connected graph, each island taken to be a vertex and each bridge taken to be an edge between two vertices. Let a Eulerian Path be a path through a connected graph that goes through each vertex precisely once. The Euler Path Theorem states that any connected graph with greater than two vertices (of odd degree) lacks a Eulerian Path. The fact that the bridges of Königsberg constitute a connected graph with four vertices with an odd degree, combined with the Euler

Path Theorem, explain why it is that it is impossible to walk across all the bridges exactly once.

The first salient feature of *Bridges* is that it is not drawn from scientific practice, unlike *Cicadas* and *Levy walks*. An adequate account of extra-mathematical explanation ought to account for these non-scientific cases, too. The second salient feature of *Bridges* is that when it is discussed, it is often unclear *quite* what the explanandum is supposed to be. The first option is that what is being explained is that one particular attempt to complete a tour of the bridges failed. The second is that *all* such past attempts to complete a tour of the bridges failed or that all future attempts will fail. The third possible explanandum is the modal fact that it is (for some sense of possibility) *impossible* to complete a tour of the bridges – this is what Pincock takes the explanandum to be (Pincock 2007; Pincock 2012). Given that an explanation of the third explanandum will trivially also count as an explanation of the first two possible explananda, this seems good reason to take the modal fact as the thing to be explained. This makes *Bridges* importantly different from the optimality cases like *Cicadas* and *Levy walks*, a third salient feature. Perhaps it is necessary that *given the adaptive conditions* the cicada evolved to have prime-numbered life-cycles but it is presumably no more necessary than that – but what is to be explained in *Bridges* is the impossibility of touring the bridge system in a particular way.

Here is a final explanation with mathematical content, adapted from Woodward (Woodward 2003: 197).

Pendulum

The period of a pendulum T has the value 2.46 (seconds). Why? The length of the pendulum L has value 1.5 and gravitational field strength *g* has value 9.81. The period of a pendulum T is related to the length of the pendulum L in the following way: $T = 2\pi\sqrt{L/g}$. This explains why, given that the length of the pendulum and the gravitational field strength have the values that they do, the period of the pendulum has the value 2.46 seconds.

Is *Pendulum* an extra-mathematical explanation like *Cicadas*, *Levy walks*, and *Bridges*? Plausibly not. This suggests a bifurcation of the notion of a mathematical explanation. (Lange provides a double pendulum example as a case of an explanation that involves mathematics that fails to be an extra-mathematical explanation (Lange 2016b: 25-26). I'll say more about this distinction in §2.8.2. below.

2.8. Criteria for assessing accounts of extra-mathematical explanation

In this section I set out some criteria that successful accounts of extra-mathematical explanation will have to meet. They will be used in the next chapter to assess recently proposed accounts and the account endorsed in chapter 4 will be argued for by demonstrating that they meet the criteria set out in this section.

2.8.1. Accounting for case studies

An obvious desideratum for an account of extra-mathematical explanation is that it sheds light on the case studies set out above. It is obviously constitutive of accounting for the case studies that, according to the successful account of extra-mathematical explanation, the case studies come out as being explanations: even minimal consistency with the judgements of experts as to what does and does not count as an explanation requires that an account of extra-mathematical explanation should not entail that *Cicadas*, for example, is no explanation at all of the primeness of the life-cycles. This is consistent, however, with diverging from any judgements made by non-philosophers about *what role* the mathematics is doing: biologists, for example, are to be trusted to identify genuine explanations of phenomena but not necessarily to make careful philosophical judgements about the contributions of this or that part of the explanations. A successful account of extra-mathematical explanation should also tell us how the *mathematical* component of the explanation functions in the explanation, too: whilst it is (near trivially) true to say that extra-mathematical explanations partially function by providing facts about the target system that are not contained in the explanandum, this weak claim tells us nothing about what the mathematics is doing in the explanation.

2.8.2. Distinguishing extra-mathematical explanations and explanations that use mathematics

As discussed above, the kinds of explanations required to buttress the enhanced indispensability argument are those in which mathematics explains a non-mathematical fact. A common sentiment is that in order to count as an extra-mathematical explanation, it is not sufficient for an explanation to merely contain some mathematics or for the explanation to express its content using mathematics (if these two come apart). Lange, for example, comments that extra-mathematical explanations are such that they differ "profoundly from ordinary scientific explanations employing mathematics" (Lange 2013a: 486) and also approvingly quotes Steiner's remark that "one senses a striking difference" between these two kinds of explanations that feature mathematics (Steiner 1978).

If any scientific explanation that featured mathematics counted as extra-mathematical then very many scientific explanations would count as extra-mathematical as very many scientific explanations are expressed using mathematics. Considering *Pendulum* will be illustrative. I will assume that *Pendulum* does not count as an extra-mathematical explanation. The mathematical statement that features in the explanation should be understood as a mathematical *expression of* a physical fact: namely, a counterfactual-supporting physical regularity.[62] It is this physical regularity that obtains between the pendulum period, the period length and the gravitational field strength that does the explanatory work, rather than a mathematical fact. *Pendulum* illustrates the fact that merely having mathematical content is not sufficient for being an extra-mathematical explanation. An explanation appealing to a mathematized law statement (for example) is not a *mathematical* explanation in the sense that Baker and Colyvan intend merely in virtue of it containing a mathematical statement (Baker 2005; Lyon & Colyvan 2009).[63]

A viable account of extra-mathematical explanation will shed light on this widely shared view that there is a difference between mathematical explanations like *Cicadas* and *Levy walks* and mathematized explanations like *Pendulum*. Given the schematic nature of accounts of extra-mathematical explanation to date, however, there is no agreed upon (or even particularly well-worked out) means of distinguishing extra-mathematical explanations from scientific explanations that use mathematics. One criterion for an account of extra-mathematical

[62] There are difficult questions about what the expressed fact comes to, given that it is plausibly a law. Assuming Humeanism about the laws of nature, the law in *Pendulum* is a worldly regularity expressed using mathematics. Although it is expressed using mathematics (making *Pendulum* a mathematical explanation in a weak sense), the law featuring in the explanation is made true by its instances – the regularities that make it true that pendulum lengths and gravitational field strengths stand in the relations expressed. Although some interlocutors found later in the thesis (such as Marc Lange) have decisively non-Humean accounts of the laws of nature, nothing said in this thesis *turns on* accepting Humeanism.

[63] Referring to equations given in accounts of coding schemes, Chirimuuta says that: "the question now is, *why should we think of this as a mathematical fact*? One reason to think that the facts summarised in the resource precision curves are straightforwardly *empirical* is that the points plotted in such graphs are the outputs of equations which have empirically measurable parameters, such as the length and width of transistors" (Chirimuuta forthcoming: 13).

explanation, then, is plausibly that it must elucidate on this distinction between mathematical explanations and explanations that use mathematics. To make this a deriseratum is not, of course, to pre-judge *how* the account ought to elucidate the distinction.

2.9. Conclusion

Two arguments have been considered in this chapter for the view that assessing the truth of the Explanation Conditional need not go via the development of substantive accounts of extra-mathematical explanation. I have argued that both arguments, the argument from inferential conservativeness and the argument from abductive maximalism, fail.

I argued that, despite it being taken for granted by many in the literature, the extent to which the mathematical realist's opponent is a scientific realist is both ambiguous and orthogonal to the substantive debate at hand. What is at issue is whether or not, in order to maintain reasoned belief in unobserved physical posits of our best scientific theories, it is necessary to endorse the form of abductive reasoning that is appealed to in the enhanced indispensability argument. I introduced the notion of inferential conservativeness and argued against the realist claim. The argument from inferential conservativeness fails. I also argued that there is no default entitlement to abductive maximalism and that, in fact, there are reasons to think that it is false. I distinguished two ways in which the debate might proceed from this perspective and argued in favour of one of them. This sets up the discussion in the next two chapters of accounts of extra-mathematical explanation.

Chapter 3

Extant accounts of extra-mathematical explanation

In the previous chapter I made the case that, because the arguments from inferential conservativeness and abductive maximalism fail, the debate about mathematics' explanatory role should be advanced by developing accounts of extra-mathematical explanation. This is in addition to the light that doing so can shed on interesting more general questions about the nature of explanation, as discussed in the introduction. In this chapter, then, I discuss accounts of extra-mathematical explanation that have been presented in the recent literature. Whilst some of the problems facing these accounts turn on their idiosyncrasies, a recurring theme is that (at least) many of the accounts fail because they cannot account in the right way for that the explananda of extra-mathematical explanations depend on the facts doing the explaining. This naturally suggests an account along the lines of the account argued for in the following chapter.

I first discuss two accounts that are anti-exceptionalist in character. I first discuss Baker's schematic extension of the deductive-nomological account of explanation and argue, amongst other faults, that it fails to distinguish between mathematical explanations of different kinds. I then briefly discuss and reject Lyon's extension of Jackson and Pettit's program account of explanation. I then discuss two accounts that appear to be *exceptionalist* in character: Lange's constraint account and Pincock's abstract dependence account. I argue that whilst these accounts successfully capture some distinctive features of the relevant set of explanations, they suffer from problems that suggest we ought to look elsewhere.

3.1. Exceptionalism and anti-exceptionalism

It will be useful to briefly recall the distinction between exceptionalism and anti-exceptionalism. An account of some kind of explanation is exceptionalist if it treats that kind of explanation as non-accomodatable by existing accounts of explanation. It is tempting, at first, to think that being exceptionalist about a kind of explanation is tantamount to holding that kind of explanation to be *sui generis*. But this is too quick. One might be an exceptionalist about extra-mathematical explanation whilst not holding it to be *sui generis*. If one held that neither extra-mathematical explanation nor metaphysical explanation (*qua* grounding) could be accommodated by standard causal accounts of explanation but nevertheless held that a single account could be given of *these* two special kinds of explanation, this would be anti-exceptionalist but would not involve taking either of the two special kinds of explanation to be *sui generis*. A kind of explanation's status as *sui generis* depends on its relation to the complete set of other kinds of explanation, whilst a kind of explanation's status as anti-exceptionalist (in the current context) depends on its relation to standard accounts of explanation.

3.2. The entailment account

A natural thought is that mathematics deals in certainties: if we use mathematics to explain some phenomenon, then the certainty present in the mathematics carries over to the explanandum. In their own ways, the entailment account and the constraint account attempt to make this intuition precise. In this section I set out the entailment account, discuss Baker's relationship with anti-exceptionalism, and discuss some of the advantages of the entailment account before arguing that it nevertheless fails.

3.2.1 Baker's diffuse anti-exceptionalism

Despite the fact that much of the debate following Baker's original presentation of the enhanced indispensability argument proceeded without a particular account of explanation in mind, the paper does in fact briefly work through some leading accounts of explanation and suggest that the case study he discusses in that paper (*Cicadas*) fits into these accounts. It is obviously false, then, to claim that *no* work has been done until reasonably recently on trying to accommodate extra-mathematical explanation into existing accounts of explanation. Indeed, this section of the Baker paper (Baker 2005: 233-236) can be treated as an argument for a kind of diffuse anti-exceptionalism: not only can extra-mathematical explanation be accommodated by *one* existing account of explanation, it can be accommodated by many!

There are a few tensions, however, between Baker's attempt in Baker 2005 and the task of this chapter and the next. The first is that Baker's purpose in briefly demonstrating the *prima facie* plausibility of *Cicadas* being accommodated by these accounts of explanation is not the production of a descriptively robust account of extra-mathematical explanation. Rather, his aim in that passage is to bolster the judgement that *Cicadas* is genuinely an explanation: that the number-theoretic theorem does explanatory work and that *Cicadas* counts as evidence that mathematical objects can play the same kind of role in our scientific theories as unobservable physical objects. In later discussions, Baker makes the case for this by appealing to the judgements to practising scientists (Baker 2009: 616). That Baker has this aim in mind is reflected in the fact that he demonstrates that *Cicadas* comes out as an explanation on many different current accounts of explanation, rather than just one. This is a sensible hedge-betting strategy if one is attempting to demonstrate that *Cicadas* is an explanation, rather than a non-explanation: if *Cicadas* is an explanation according to many accounts of explanation then, no matter which of these ends up being vindicated by separate debate in the general philosophy of science, the result that *Cicadas* is an explanation is secured. However, if the aim is to pinpoint how extra-mathematical explanation *actually functions*, to pinpoint *in virtue of what* the mathematical fact plays a role in the explanation, demonstrating that *Cicadas* is consistent with many accounts of explanation does not get us very far.

Finally, that Baker's aim in this passage is not to provide a descriptively adequate account of how extra-mathematical explanation is reflected in the fact that he provides reasonably shallow glosses of the kinds of explanation he discusses. This is reflected most clearly in his rejection of accommodating extra-mathematical explanation by appealing to causal accounts of explanation. Baker notes that "according to the causal account, explaining a phenomenon involves giving a description of its various causes" and claims that this is a defeater for any attempts to accommodate extra-mathematical explanation into a causal account of explanation because "mathematical objects (if they exist) are acausal" (Baker 2005: 234). At a suitably high-level of abstraction it is, of course, true that current causal accounts of explanation claim that explanation involves giving a "description of [the explanandum's] causes" but Baker does not attempt to discuss the details of how any contemporary causal accounts of explanation spell this notion out. I discuss this point in much more detail in the next chapter, so will say no more here: this is raised here to motivate developing the project of the Baker passage in more depth, the task of this chapter.

In addition to his brief discussion and subsequent rejection of causal account, Baker sets out how *Cicadas* can be accommodated by the pragmatic account of explanation and the D-N model of explanation. Pragmatic explanation seems ill-suited to licensing inference to the existence of an object on the basis of it being purportedly referred to by the premise of an explanation. So, in this section, I take Baker's proposed extension of the D-N account of explanation (as offered to bolster the judgement that *Cicadas* is genuinely an explanation) and see how it fares as a descriptively adequate account of extra-mathematical explanation.

3.2.2. The D-N account of explanation

According to the D-N account of explanation, explanations take the form of deductive arguments. These arguments take as their conclusion the explanandum and as their premises the explanans. The following conditions are placed on an argument counting as an explanation of its conclusion. First, the explanandum must be a logical consequence of the explanans. Second, the explanans must contain at least one law and the law(s) must be essential to the explanation (where for a law to be essential to the explanation is for the other premises to not deductively entail the conclusion). Finally, the explanans must have empirical content and the explanans/premises must be true.

As is well known, the D-N account of explanation suffers from problems: I will only very briefly rehearse them here. The first problem is that of explanatory asymmetry. There are pairs of deductively valid arguments that appeal to the same law(s), satisfy the requirements and where an explanandum of one argument is an explanan of the other. However, it is true of these argument pairs that only one of the arguments is intuitively an explanation.[64] The second problem is that of explanatory irrelevance. In these cases, an argument can be given that satisfies the requirements yet with premises that seem in some sense irrelevant to the conclusion/explanandum.[65] In short, then, the D-N account is taken to be unsuccessful in virtue of there being deductive arguments that satisfy the conditions even though the explanans fail to jointly explain the explanandum.

It is tempting, then, to reject a potential entailment account of extra-mathematical explanation at this point. A competing consideration, though, is that there are some *prima*

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[64] The canonical example: the length of a flagpole's shadow can be derived from information about the height of the flagpole, the angle the flagpole makes with the sun and laws governing the propagation of light; the height of the flagpole can be derived from information about the length of the shadow, the angle the flagpole makes with the sun and laws governing the propagation of light. Yet, it is possible to explain the shadow length by citing the information about the height, angle and laws but intuitively impossible to explain the height by citing information about the shadow length, angle and laws.

[65] A deductive argument can be provided that has as its explanandum that some hexed salt dissolved and that has as its premises: the salt was placed in water, all hexed salt dissolves and that the salt was hexed. Yet, it is clear that the salt being hexed does not explain its dissolution (Salmon 1984: 94).

facie reasons for thinking that extra-mathematical explanation is amenable to a treatment along deductive-nomological lines. The standard portrayal of the *Cicadas* explanation, appealed to by both supporters (Baker 2005) and critics (Saatsi 2011) of its use in the enhanced indispensability argument, is indeed in terms of premises and conclusions. Second, as seems to be the case in at least some of the case-studies considered, extra-mathematical explanations plausibly demonstrate that *given the mathematical fact,* the explanandum must obtain. An account of explanation with deductive entailment seems a way of accommodating this distinctive feature of extra-mathematical explanation. Furthermore, an entailment account being inappropriate for understanding some kinds of explanation doesn't automatically mean that it cannot be used to understand some other kind of explanation. Finally, there are interesting problems with the entailment account *as an account of extra-mathematical explanation* which can help guide us to a successful account.

3.2.3 The entailment account of extra-mathematical explanation

In this section I set out the entailment account of extra-mathematical explanation. Baker presents *Cicadas* in the form of a deductive argument with the explanandum as its conclusion and the number-theoretic theorem featuring amongst the explanans. Even though this is not the form in which the explanation is given in the biological literature (Yoshimura 1997; Goles, Oliver & Markus 2001), this needn't be a serious problem.[66] As noted above, Baker presents *Cicadas* in a schema that resembles the kind of argument that, according to the D-N account, constitutes an explanation. It's ambiguous, though, that *Cicadas* cites any laws. Here are the most plausible candidates:

> Having a life-cycle period which minimises intersection with other (nearby/lower) periods is evolutionarily advantageous.
>
> Prime periods minimise intersection.

Baker notes the possible lack of laws in *Cicadas* and responds in the following way:

> One point in the platonist's favour is that the purely mathematical premise of the cicada inference is in the form of a general law, in this case a theorem of number theory. A

[66] Assuming some reasonable account can be given of the relationship between the deductive argument and what is found in the biology literature.

> broadening of the category of laws of nature to include mathematical theorems and principles, which share commonly cited features such as universality and necessity, would count the mathematical theorem (2) as explanatory on the same grounds as the biological law (1). (Baker 2005: 235)

This move reflects the fact that the law-like statements featuring in D-N explanations do not play their role in the explanation in virtue of the lawfulness, but in virtue of their generality. The suggestion offered by Baker, then, is to weaken the second requirement of the D-N account, I assume such that it now says one of the following:

> 2*. The explanans must contain at least one law or mathematical theorem and the law(s) and/or mathematical theorem must be essential to the explanation (a law is essential to the explanation if the other premises alone do not deductively entail the conclusion).
>
> 2**. The explanans must contain at least one premise that holds universally and/or with necessity and this premise must be essential to the explanation (a premise is essential to the explanation if the other premises alone do not deductively entail the conclusion).

3.2.4. Problems with the entailment account

3.2.4.1. Empirical content and independent support

That is the solution to the problem raised (and solved) by Baker. Here, however, is a second problem. The number-theoretic theorem (and the Euler Path Theorem and the claim about twenty-three's divisibility) lack empirical content. On the surface, then, the requirement that each explanan must have empirical content is a decisive objection to the entailment account: mathematical facts are not the right kind of thing to have empirical contents.[67] However, one of the motivations behind this requirement is that each explanan must have received independent support. Given this motivation, the proponent of the entailment account could

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[67] As has been pointed out this issue is slightly complicated in some of the cases: the number-theoretic theorem in *Cicadas*, for example, is already couched in terms of 'periods', an empirical concept (Baker 2017: 194). Lange offers related explanations for the standard optimality explanations that are he claims are the *genuine* extra-mathematical explanations (Lange 2013a: 499). I return to Lange's comments about what the distinctively mathematical explanation is in the cicada case in fn.87.

suggest the following fix. The proponent of the entailment account of extra-mathematical explanation could suggest that the requirement that the premises can be given some independent support should be widened to include support from mathematical practice. That is: even if the number-theoretic theorem cannot receive empirical support (at least, not in the usual way), in virtue of lacking empirical content, a modified version of the entailment account could permit that independent verification of the premises of an explanation can come from mathematical practice, in addition to observation. On this line, cases of extra-mathematical explanation are not like faulty non-explanations that contain premises that cannot receive independent support in virtue of lacking empirical content: rather, we have good reason to believe that prime periods minimise rather than maximise intersection. Indeed, we have a *proof* that prime periods minimise intersection, rather than having some other effect on the chance of intersection.

There is, however, a problem with this extension of the entailment account. At least two kinds of commentators partaking in this debate will demur from the claim that the number-theoretic theorem gets independent support from mathematical practice. First are realists who claim that the only good reasons we can have for believing a mathematical claim stem from mathematical practice (Colyvan 1998: 39). If we accept that the number-theoretic theorem gets independent support from mathematical practice, then the indispensability argument begins to seem surplus to requirements. This leads to a more pressing concern. Many nominalists will simply reject the claim that the number-theoretic theorem receives independent support from mathematical practice (at least, if the support is understood as being reason for thinking that the theorem is *true*).[68] This leads to subtle concerns regarding question-begging.

Of course, one desired consequence of developing an account of extra-mathematical explanation is that it might shed light on the mathematical ontology question in a way that is principled. Is it a problem, then, that a modified entailment account must assume that the mathematical fact appealed to by an extra-mathematical explanation is true? I think so. If either the realist or nominalist is to use the correct account of extra-mathematical explanation to argue in favour of their view, their argument must be of the form: there are reasons for preferring x that hold for all reasonable participants of the debate, regardless of one's

[68] I say *many* nominalists will balk at this claim (rather than *all*) because of the (at least, logical!) possibility of being a nominalist in the sense of holding that there are no mathematical objects but a truth-value realist – holding that the sentences of mathematics are (sometimes) true.

ontological aim, and it is a consequence of *x* that *y* (where *x* is the correct account of extra-mathematical explanation and *y* is some view in the metaphysics of mathematics).

3.2.4.2. Irrelevance and mathematical explanations of different kinds

The extended entailment account, then, is plausibly question-begging. Here is a second pair of problems for the entailment account. On the kind of account being considered here, a mathematical sentence is explanatory if it features in a deductive argument with the explanandum as its conclusion. However, not all mathematical sentences will be relevant to the conclusion but all mathematical sentences could be a premise of a deductive argument that has as its conclusion the explanandum, given the necessity of mathematical facts. A mathematical fact that is intuitively irrelevant to the explanation could be added to an extra-mathematical explanation and count as explanatory. The problem of explanatory irrelevancies is, of course, one of the standard objections to the D-N account of explanation – so far, then, there seems nothing *special* about this problem.[69]

However, there is a deeper, related problem. Recall *Pendulum*, an example of an explanation that had mathematical content without counting as an extra-mathematical explanation. What is wrong with *Pendulum* is not that the pendulum law is irrelevant to the explanation.[70] Indeed, any account of relevance that had this consequence would be faulty. The mathematically stated pendulum law surely counts as relevant to the explanation: it is therefore a relevant piece of mathematics. However, *Pendulum* is not an extra-mathematical explanation. As it stands, the entailment account fails to distinguish between mathematical and mathematicised explanations. In both kinds of explanation, on the entailment account, the mathematical sentence is performing the same role: logically entailing the explanandum. As it stands, I do not think extending the entailment account is a viable way of understanding extra-mathematical explanation.

[69] It is tempting to appeal to this modal character of both entailment and the mathematical fact to run a version of the asymmetry objection. Assuming that mathematical facts are necessary if true, then by swapping the number-theoretic theorem in *Cicadas* with the explanandum generates an explanation of the number-theoretic theorem, according to the entailment account. However, the resulting (purported) explanation of the number-theoretic theorem is not an extra-mathematical explanation, given that the explanandum is a mathematical fact.

[70] This objection would also apply to a version of the entailment account that is supplemented with a suitably updated account of relevance (*cf.* Baron 2016b: 20-23).

3.3. The program account

Extending the D-N account, then, does not seem a fruitful way of understanding extra-mathematical explanation. In this section I briefly discuss another recent anti-exceptionalist account, Lyon's program account of extra-mathematical explanation.

Lyon (Lyon 2012) develops a suggestion of Colyvan's that extra-mathematical explanation could be accommodated by Jackson and Pettit's (Jackson & Pettit 1990b) program account of explanation. Jackson and Pettit contrast two different explanations of the same explanandum, one of which is taken to be a process explanation and the other a program explanation. Consider Putnam's (Putnam 1975: 295) two explanations of the fact that a square peg fails to fit through a round hole when the hole has a diameter equal to the side length of the peg, one that cites a particular microphysical story about parts of the peg touching parts of the board and one that cites the squareness property of the peg and the roundness property of the hole. The former is a *process* explanation and the later a *program* explanation. Lyon characterises a program explanation as one that

> cites a property or entity that, although not causally efficacious, ensures the instantiation of a causally efficacious property or entity that is an actual cause of the explanandum (Lyon 2011: 8)

The basic idea is that a program explanation does work by providing information about the facts that ensure that (or program for, hence the name) there is a causal process that brings about the explanandum: the programming properties ensure the presence of the causally efficacious properties that would be cited in a process explanation of the same explanandum event. Jackson & Pettit say that the programming properties are causally relevant but not causally efficacious: as noted by Lange, then, on this account extra-mathematical explanation comes out as causal, in this narrow specialised sense (Lange 2013a: 506).

Lyon, discussing some cases of extra-mathematical explanation, says that they are program explanations:

> [The explanations] cite properties and/or entities which are nor causally efficacious but nevertheless program the instantiation of causally efficacious properties and/or entities that

> causally produce the explanandum. And, importantly, they cite mathematical properties and/or entities that are doing (at least part of) this programming work. (Lyon 2012: 9)

Lyon briefly works through the examples, suggesting of each that the mathematics is doing programming work. Of *Cicadas*, for example, he says that "a detailed process explanation could be given for why the *Magicicada* have life-cycles of 13 and 17 years" (*ibid*) but that this would be unsatisfactory compared to the explanation actually given by the biologists.

3.3.1. Problems with the account

Saatsi (2012) raises the following objection to Lyon's account.[71] Jackson and Pettit's relationship between a program explanation the lower-level explanation that it programs is a relationship between some higher-level programming properties and a particular causal process that they (in some sense) necessitate. As such, the account is suited only to explaining events rather than regularities: it's unclear how we should make sense of the idea of a causal process that leads to (and therefore explains) a regularity. Whilst there will be a particular causal process that brought about this or that particular individual cicada having the life-cycle length that it did, what is to be explained is the regularity. According to Saatsi, the program explanation is not suited to such explananda. Speaking of the *Bridges* regularity, Saatsi says that "it is difficult to make sense of the notion that there exists any corresponding process explanation of this very explanandum" (Saatsi 2011: 581) – the process explanation needs to be a particular causal process that *brings about* the fact that no-one has ever completed a tour of the bridges, which is a regularity.

There is a response that Lyon could give to this – but it is only partial. As Saatsi notes, one way to present the explananda of optimality explanations is in the form of regularities: the regularity that cicada life-cycles are always prime, the regularity that sharks and other marine animals move in particular patterns and so on. But it seems equally natural, in these cases, to think that each fact constituting the regularity is grounded by another fact: the fact that

[71] In the same discussion note, Saatsi discusses two additional objections that I do not discuss here. The first is a point of dialectical permissibility (in short: the worry is that Lyons assumes, rather than argues, that by playing a programming role the mathematics is doing explanatory work rather than representing an explanatory non-mathematical property) and the second expresses scepticism about the possibility of giving a full account of how a mathematical property could do programming work of the kind appealed to in Jackson and Pettit's account. The first objection does not concern the viability of Lyon's account as a *descriptively adequate* account of extra-mathematical explanation and I share Saatsi's scepticism that forms the basis of the second objection (See Saatsi 2012: 581-582 for details). Lyon simply does not say enough in this regard.

cicadas *evolved* to have prime-numbered life-cycles and the fact that sharks *evolved* to have the particular trait (and so on). *These* events, unlike the regularities that they bring about, do have an underlying causal explanation, and this is already implicit in the explanations. Lyon could claim in response to Saatsi that both the optimality explanations and the peg and board explanation do their explanatory work by not including detailed causal information but only by including information about the facts that necessitate that one of the many possible causal processes took place, each of which have as their outcome the event of the trait being selected for.

Although this response seems promising, it fails to generalise properly. Consider the three possible explananda in *Bridges:* that a particular attempted tour of the bridges failed, that all such attempts have failed and that all such attempts *necessarily* fail. One of the explananda in *Bridges* is the modal fact that crossing the bridge in the specified way is impossible (for some sense of necessity). The problem (in at least some of the cases), then, is not that the explanandum of an extra-mathematical explanation is a regularity but that it is the wrong kind of fact to be explained by the kind of causal process that programming properties program for. What is the event in *Bridges* that is analogous to the evolution of the trait in optimality explanations, the event that grounds the regularity (the constant failings of attempts to tour the bridges)? Perhaps Lyon could claim that the one causal process that led to the regularity is (something like) the *building* of the bridge system such that it has the graph that it has and that this grounds the regularity. But this has things backwards: whatever the programming property is in the *Bridges* case, it doesn't seem like it could have programmed *for the building of the bridge*. If the causal process that led to the regularity is the building of the bridge system, this doesn't seem to be the right kind of causal process to have been programmed for by the higher-level graph-theoretic properties.[72] Combined with the additional worries raised (see fn.71), Lyon's account seems to be problematic.

An instructive aspect of Lyon's account, as with Baker's, is that it attempts to use an existing account of explanation to shed light on the extra-mathematical cases: this is clearly along the right lines. However, the program account *in particular* does not seem a suitable candidate. Perhaps inspired by the failure of this anti-exceptionalist account, Lange and Pincock offer (what seem to be) exceptionalist accounts. Those are inspected in the next two sections.

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[72] Lyon could, of course, deny that *Bridges* is an extra-mathematical explanation, or narrow his account to the optimality examples: but the former seems question-begging and the latter sacrifies generality.

3.4. The constraint account

In a recent paper, Lange discusses an account of extra-mathematical explanation that takes as central the idea that such explanations (in some sense) have greater modal force than other scientific explanations: in a sense hopefully to be made precise, extra-mathematical explanations constrain the range of possibilities. This sharply contrasts with explanations that explain by describing the world's causal powers. It is tempting, then, to straightforwardly cast the constraint account as exceptionalist: as a kind of explanation that cannot be accommodated by existing accounts of explanation. This understanding of the constraint account is consistent with what Lange says in his original presentation of the account (Lange 2013a). However, the easy categorisation of Lange's account as exceptionalist is complicated by the fact that in an overview on his recent work on non-causal explanation, Lange says that distinctively mathematical explanations belong to a wider group of explanations: explanations by constraint (Lange 2016b: *xvi*). The notion of constraint appears once in the earlier version of the paper in which he sets out the constraint account but he does not signal that he holds extra-mathematical explanations belong to a wider class of explanations that all explain in this way. Lange says that:

> [S]ome non-causal scientific explanations work by identifying certain constraints to which the world must conform. These constraints (such as mathematical facts and symmetry principles) apply to causal processes, but not in virtue of their being causal processes (*ibid*)

Perhaps, however, the anti-exceptionalist understanding of the constraint account of extra-mathematical explanation survives. Recall that one helpful way of unpacking the notion of anti-exceptionalism cleaved it apart from holding an explanation being *sui generis*. Being an exceptionalist about a kind of explanation plausibly means that it cannot be accommodated by existing accounts of explanation – and this is clearly Lange's view, given that he mentions Woodward and Strevens' accounts in passing and claims that extra-mathematical explanation cannot be accommodated by them (*ibid*: 487). What Lange's latest comments reveal is that he holds that extra-mathematical explanation belongs to a more general kind of explanation, the explanations by constraint. Even if there is some general account called 'the constraint account' that can accommodate *all* such explanations (with the extra-mathematical explanations being only one kind), Lange is still an anti-exceptionalist in virtue of the fact that he thinks some *new* account of explanation must be given in order to accommodate the extra-mathematical cases. As will become apparent in chapter 4, however one carves up the exceptionalist/anti-exceptionalist distinction, the fundamental disagreement between my view and Lange's remains. Before this can become apparent, it is necessary to set out Lange's view and raise a problem with it.

This idea of extra-mathematical explanations demonstrating that the explanandum is necessary (for a strong sense of 'necessary') is crucial to the constraint account. Lange gives the following summation of his account:

> Such an [extra-mathematical] explanation works […] by (roughly) showing how the fact to be explained was inevitable to a stronger degree than could result from the causal powers bestowed by the possession of various properties. If a fact has a distinctively mathematical explanation, then the modal strength of the connection between causes and effects is insufficient to account for that fact's inevitability.
>
> These explanations work not by describing the world's network of causal relations in particular, but rather by describing the framework inhabited by any possible causal relation. (*ibid*: 509)

Lange provides the example of Newton's second law as an example of what he means by 'framework': the law does not give information about a particular cause, instead being "the framework within which any force must act" (*ibid*: 503). Something cannot be a force without satisfying Newton's second law, the thought goes. Extra-mathematical explanations give information about the constraints on possible behaviour. Lange is at pains to point out that the notion of possibility here is stronger than nomological necessity: "these necessities are stronger than causal necessity, setting distinctively mathematical explanations apart from ordinary scientific explanations. Distinctively mathematical explanations in science work by appealing to facts […] that are modally stronger than ordinary causal laws" (*ibid*: 491). The idea, it seems, is that extra-mathematical (or 'distinctively' mathematical) explanations do not turn on any causal laws that are true at the actual world or at any world. In *Bridges*, for example, there is no causal process (consistent either with the actual causal laws or any possible causal laws) that would render the Königsberg bridge system tourable: the explanation does not appeal to any physical laws or facts. These physical facts are contained, for Lange, in the presupposition: referring to *Bridges* he says that there are "various contingent facts *presupposed* by the why question that the explanandum answers, such that the arrangement of the bridges and islands is fixed" (*ibid*: 506) (emphasis added). The degrees of necessity present in distinctively mathematical explanations and regular causal explanations differ, according to the constraint account, and this fact distinguishes them.

It is clear that Lange means that *both* the explanandum of an extra-mathematical explanation *and* the mathematical fact that features in the explanation hold with a kind of necessity not found in the explanandum or any of the explanans in standard explanations:

> These explanations explain […] by revealing that the explanandum is *necessary* – in particular more necessary than ordinary laws of nature are. (*ibid*: 491)

> Distinctively mathematical scientific explanations work by appealing to facts (including but not always limited to mathematical facts) that are modally stronger than ordinary laws of nature, together with contingent conditions that are contextually understood to be constitutive of the arrangement or task at issue in the why question. (*ibid*)

I take it, then, that there is a sense in which the explanandum inherits the necessity of the explanan that has the special modal status. There is a sense in which it seems infelicitous to say that the explanation works by "revealing that" the explanandum is necessary: in *Bridges*, for example, before the explanation is given to us it is apparent that it is impossible (for some sense of 'possible') to cross every bridge, crossing every bridge only once (to complete a tour of the bridge system). Indeed, it seems like *what is to be explained* is that it is impossible to cross the bridges in this way: this is why an explanation appealing to the particularities of the bridge system won't do, even though such an explanation might demonstrate *that* it is impossible to complete a tour.

3.4.1. Advantages of the constraint account

Despite the fact that it's somewhat hard to pin down the explicit commitments of the constraint account, there are many things that it seems to do well. First, and most obviously, it accounts for the sense in which many extra-mathematical explanations demonstrate that their explananda hold with (some kind) of necessity and does so in a way that makes more precise how this fact relates to the necessity of the mathematical fact that features in the explanation. Another strength is that it provides criteria for distinguishing between mathematized explanations, like *Pendulum*, and extra-mathematical explanations, like *Bridges*, and does so in a way that is not ad-hoc and follows naturally from the details of the account. Recall that this requirement was to explain in a principled way whether or not the pendulum law that features in *Pendulm* and the graph-theoretic fact that features in *Bridges* (for example) do explanatory work in different ways: this seems to be required to respect the judgement that not *all* explanations that feature mathematics are extra-mathematical explanations. The constraint account can explain this disparity by pointing out that the graph-theoretic fact (and the number-theoretic theorem in *Cicadas*, and so on) holds with a kind of necessity stronger than physical necessity whilst the pendulum law that features in *Pendulum* does not. Plausibly, the relationship between length and gravitation field strength (and so on) could have been different, if relevant other features of the world had been different in the appropriate ways: that is, the mathematics appealed to in the pendulum law could (if things had been different) have expressed a *false* generalization that would have failed to do explanatory work.[73] This contrasts with the graph-theoretic fact, which could not have been otherwise: there is no world at which a bridge system with a Eulerian graph is crossable in the specified way. Lange's account, then, gives the resources to distinguish explanations that use mathematics from extra-mathematical explanation: whether the mathematics is used to express a contingent fact (and so does not demonstrate that the explanandum holds with a form of necessity higher than physical necessity) or whether the mathematics is, itself, a fact that holds with necessity. Indeed, Lange says something suggesting this line of thinking when he discusses the following explanation:

> Why are all planetary orbits elliptical (approximately)? Because each planetary orbit is (approximately) the locus of points for which the sum of the distances from two fixed points is a constant, and that locus is (as a matter of mathematical fact) an ellipse. (*ibid*: 508)

Lange rejects this as an example of a distinctively mathematical explanation (or extra-mathematical explanation), in virtue of the fact that "the first fact to which it appeals is neither modally more necessary than ordinary causal laws nor understood in the why question's context to be constitutive of being a planetary orbit (the physical arrangement in question)" (*ibid*).

[73] Whilst the claim that the laws of nature (at least, non-fundamental laws like the pendulum law) are contingent and so could have been otherwise seems to be widely held, it is by no means uncontested. See, for example, Bird's arguments that (at least some) laws of nature are necessary (Bird 2004).

3.4.2. The constraint account and contextual presuppositions

There are, however, problems with Lange's account that prompt us to look elsewhere. In this section I set these problems out and consider possible responses.

A distinctive aspect of Lange's account the role of the explanatory presupposition and that he is relatively open as to what facts get to go into this presupposition. Reutlinger notes that this contextual aspect of the constraint account might render the distinction between mathematical and non-mathematical explanations non-sharp (Reutlinger 2017a: 8).[74] This point is well taken – although I am not sure this is as serious a problem as Reutlinger (and Pincock) take it to be. Even if this renders extra-mathematical explanations abundant, it will not entail that very many explanations *offered in scientific practice* count as extra-mathematical. As will become clear, that the constraint account renders this boundary non-sharp or renders extra-mathematical explanation abundant is not the objection being pressed here. Rather, here, the worry is that the contextual aspect of the account can render as explanatory cases that we judge to be non-explanatory. This, I take it, is a more pressing worry than the potential rendering of the mathematical/non-mathematical boundary as non-sharp.

Suppose we, as we might do, wish to explain why it is that the ecological constraints in a particular cicada ecosystem are such that they limit possible life-cycles to lengths within a certain range. Suppose that, as is the case, we know facts about what life-cycles cicadas have and that prime periods minimise intersection. It seems that these facts, including the number-theoretic theorem, place constraints on our new explanandum, the life-cycle range. If the non-mathematical facts are taken to be presupposed in the given context, the number-theoretic theorem seems to explain the ecological constraints. But, this seems incorrect. As noted above (and as discussed in fn.87 below) Lange disputes that *Cicadas,* as it is normally formulated, is an extra-mathematical explanation. Here, then, is a way to press the problem with an uncontroversial example. Suppose we know, as we do, that it is impossible to complete a tour of the bridge system – this is the thing that are trying to explain, so it certainly counts as amongst our evidence and propositions known. Suppose we also know, as we do, that it is impossible to complete a tour of a bridge system if the system constitutes a graph with an odd number of vertices. This, too, is something that we know: this is just the graph-

[74] See, also, Pincock's remark that "the danger is that every phenomenon will have a distinctively mathematical explanation" (Pincock 2015: 875).

theoretic fact that plays a role in *Bridges*. These two facts, however, seem to place constraints on what the number of vertices are had by the graph constituted by the bridge system in just the same way as in the original explanation, and the explanan with the special modal status (the graph-theoretic fact) plays the same kind of role. But, of course, we intuitively cannot explain why it is that the graph constituted by the bridge has the number of vertices that it does in this way. A genuine explanation of this fact would have to cite facts about the bridge's construction, for example. Nevertheless, if the non-graph-theoretic facts are placed in the presupposition, it seems as if the graph-theoretic fact explains the explanandum, according to the constraint account.

It is tempting to present this worry about the presupposition in a way that mirrors the asymmetry objection to the D-N account of scientific explanation that was discussed in §3.2, but I think this is a mistake. In a recent paper, Craver and Povich do this (Craver & Povich 2017), reorganising the explanans and explanandum of some extra-mathematical cases as is standard in formulating the asymmetry objection to the D-N account of explanation. However, to do this makes some strong assumptions about what the canonical form of an explanation is on the constraint account, an issue that Lange is silent on. Furthermore, it flattens the different roles played by each of the facts involved in the explanation. On the objection presented above, some facts are placed *in the presupposition*, and this makes it the case that the mathematical fact is the sole fact doing explanatory work (in line with Lange's understanding of (what he calls) distinctively mathematical explanations). The worry is, as pressed above, that the mathematical fact *doesn't* explain the explanandum, even with the relevant facts placed in the presupposition. Taking each fact as if it is a premise in an explanatory argument (with the argument having both empirical and mathematical premises) as Craver and Povich do, does not seem to respect this aspect of the constraint account. The fact that some of (what we would naturally call) the explanans are *presuppositions* (contextually determined) whilst one of the explanans is *the fact doing the explaining* suggests that one should not take explanations by constraint to take the form of arguments with the explanandum as their conclusions. The role of some facts being placed in the presupposition is an important component of Lange's account: otherwise it seems to collapse into the entailment account, discussed above. The fact that certain facts are placed in the presuppositions is what *makes* the constraining fact explanatory.

However, what this understanding of the worry *does* get right is that one similarity does carry over from asymmetry worries about the D-N account. In the flagpole case, a very natural response is that we cannot explain the height of the flagpole by citing facts about the length of the shadow because the length of the shadow does not depend on the height of the flagpole in the right kind of way. This 'right kind' of dependence is normally understood causally: the height of the flagpole plays a role in causing the shadow to have the length that it does, but the length of the shadow plays no role in causing the flagpole to have the height that it does. There is a similarly natural response, here. However, it's less than clear how Lange's notion of constraint could accommodate this way of thinking: once the relevant facts are placed in the explanatory presupposition, the single (intuitively non-explanatory) fact does seem to generate constraints, such that the explanandum is necessitated.

Here is one possible response that *could* be made on Lange's behalf. Lange might argue that his account in fact *can* accommodate the fact that the alternate *Bridges* explanation is no explanation at all and can do so without resorting to dependence talk. The constraint account assigns a crucial role to the fact that the explanandum holds with a greater degree of necessity than physical necessity. I suggested above that the account could be understood as claiming that an explanation demonstrates *why* the explanandum holds with greater-than-physical necessity rather than demonstrating *that* it does. Lange could, therefore, respond by claiming that the proposed explanandum in the problematic case (that the bridge system has an odd number of bridges) doesn't hold as a matter of necessity and, therefore, this explanation does not count as an explanation-by-constraint.

I don't think this response is successful. It seems that the fact that the bridge system having the number of bridges that it does holds with the same kind of necessity as the fact that the bridge system is non-tourable. What is necessary, I take it, is not that *the bridge system in Konigsberg is non-tourable*: if the bridge system in Konigsberg had a different number of bridges, then it would have been tourable. What is necessary, instead, is that *given that it has an odd number of bridges,* the bridge system in Konigsberg is non-tourable. Without this conditional aspect, there is no necessary fact at all. It is true, I take it, that the bridges of Konigsberg could have been tourable but it is not possible that *this bridge system* is tourable. There is a directly analogous pair of necessary and merely contingent explananda in the problematic case. It is possible, as the proposed rejoinder from Lange goes, that the bridge system could have had a different number of bridges. But it is impossible that, *given the graph-theoretic fact* *and the fact that the bridge system is non-tourable*, the bridge system could have had an even number of bridges. The graph-theoretic fact, on this reading, equally plays a role in constraining the number of bridges that make up the bridge system: that is, it constrains it to being an odd number. Contra the constraint account, however, the fact that the graph-theoretic fact and the fact about non-tourability constrain the number of bridges in this way, and in turn demonstrate *that* it is necessary that the bridge system has an odd number of bridges, does not explain *why* the bridge system has the number of bridges that it does. Again, the reason for this seems simply to be that there is not the right kind of dependence between the tourability, the graph-theoretic fact and the number of bridges.

At first, Lange's account seemed to be on the right track: it successfully met some of the criteria discussed in chapter 2. However, the notion of explanation by constraint seems too liberal: it is *too easy* to explain in the way set out by Lange's account, and as a result non-genuine explanations ended up counting as explanations by constraint. I suggested a way of correctly rendering the non-explanations as faulty, but also suggested that Lange's account cannot straightforwardly accommodate this.

3.5. The abstract dependence account

In this section I set out Pincock's account of extra-mathematical explanation, which I will refer to as the abstract dependence account.

In a recent paper, Pincock sets out what appears to be an exceptionalist account of extra-mathematical explanation: like Lange, he explicitly suggests that the kind of explanations he is concerned with cannot be accommodated by standard accounts of explanation (Pincock 2015: 867). There are preliminary uncertainties when it comes to Pincock's account. First: the most recent discussion of such explanations (Pincock 2015), Pincock refers to the class of explanations under discussion as 'abstract explanations' rather than extra-mathematical explanations. However, Pincock uses the term 'mathematical explanation' on occasion (Pincock 2015: 871) and also contrasts his account with Lyon's account (*ibid*: 871) and Lange's account (*ibid*: 874). Second: in addition to his most recent discussion, Pincock also discusses extra-mathematical explanation in three other places (Pincock 2004b; Pincock 2007; Pincock 2012). In the most recent paper, Pincock does not explicitly say how the most recent statement of his view relates to his earlier views. Perhaps, like Lange's notion of a constraint explanation, Pincock holds extra-mathematical explanations to be a subset of the abstract explanations. On this understanding, although extra-mathematical explanations cannot be accommodated by standard accounts of explanation, they are nonetheless not *sui generis*. This ambiguity, although interesting, does not bear on the reasons for which Pincock's account is unsatisfactory. In this section I try to set out the core commitments of Pincock's view, drawing on his comments across his various discussions. Even if there isn't a precise formulation of the view that is consistent with everything Pincock says, in this section I am interested in assessing whether there is a plausible view that draws on the core insight of all of Pincock's discussions, drawing together two strands: the notions of abstract dependence and the irrelevance of microphysical details.

In his most recent account of his view on this issue, Pincock says the following of his view:

> We think we have an explanation when we have found a (i) classification of systems using (ii) a more abstract entity that is (iii) appropriately linked to the phenomenon being explained. Whenever an explanation has these three features I will say that we have an abstract explanation. (Pincock 2015: 867)

What does Pincock mean by 'abstract entity'? The use of the term "*more* abstract entity" (emphasis added) suggests that Pincock does not mean an abstract object in the standard sense (seeing as the standard sense of abstractness does not admit degrees, at least when being used to characterise the abstractness *of an object*), yet the rest of what he says *does* seem to cohere with the standard sense. Pincock does say this, which is instructive:

> Note that a distinguishing feature of abstract explanation and abstract dependence is that we appeal to a more abstract entity that has a more concrete entity as an instance. (*ibid*: 879)

A second natural question about the account is: what, exactly, is abstract dependence? Pincock does not provide a clear definition of this notion. For the target system to abstractly depend on the entity invoked is for the abstract entity to be "appropriately linked" to the explanandum. Pincock also takes the concrete physical system under consideration to be an "instance" of the abstract entity: "we can say that a soap-film surface obeying Plateau's laws depends on its being *an instance of* an almost minimal set" (*ibid*: 869). On this understanding, abstract extra-mathematical explanations are distinguished by the fact that the explanans serve as an explanation for any explanandum physical system that realizes (or is an instance of) the kind of abstract system described in the explanans. This is the sort of picture that Pincock seems to have in earlier discussions of the *Bridges* case.

> The abstract explanation seems superior because it gets at the root cause of why walking a certain path is impossible by focusing on the abstract structure of system. Even if the bridges were turned into gold, it would still have the structure of the same graph, and so the same abstract explanation would apply. By abstracting away from the microphysics, scientists can often give better explanations of the features of physical systems. (Pincock 2007: 260)

Bringing these earlier comments in line with Pincock's recent precis of his account quoted on the previous page: *Bridges* does its explanatory work by classifying potential bridge systems by using a more abstract entity than the system under consideration (the more abstract entity in this case, I take it, being the graph) that is appropriately linked to the phenomenon (in that the physical bridge system is an instantiation of the abstract graph). However, I take it that if all Pincock meant by 'abstract dependence' and 'linked in the right way' was instantiation or realisation, he would have said so. Whatever one thinks about the multiple ways in which these terms are used in philosophy, there is a better collective grip on them than there is to be had on being 'appropriately linked' – in addition, in concluding his discussion Pincock is unsure and noncommittal regarding the nature of abstract dependence.[75] Perhaps the most reasonable understanding of Pincock's account is that there are *many* ways in which the abstract entity can be appropriately linked (and therefore multiple ways in which a target system can abstractly depend on the entity) to the phenomenon being explained: that being 'appropriately linked' is *something like* a functional role and realizing and instantiating are relations that can play this role in bringing about an abstract explanation in particular cases. This interpretation respects both Pincock's use of the term 'instances' but also the fact that he remains non-commital regarding the nature of abstract dependence.

In addition to the key role played by abstract dependence, there is a second aspect of Pincock's account of extra-mathematical explanation that is worth stressing. Earlier expositions stress the fact that explanations like *Bridges* purportedly do not contain information about microphysical details of the target and that this is (partially?) constitutive of the information being explanatory: "the explanatory power is tied to the simple way in

[75] See, for example, his remark that "at least two things are needed to flesh out this suggestion. First, there should be a theory of what abstract dependence comes to and how these relations are distributed in the world" (Pincock 2015: 877).

which the model abstracts away from the irrelevant details of the target system" (Pincock 2011: 213). For example:

> [A]n explanation for this [that it is impossible to cross each bridge exactly once] is that at least one vertex has an odd valence. Whenever such a physical system has at least one bank or island with an odd number of bridges from it, there will be no path that crosses every bridge exactly once and that returns to the starting point. If the situation were slightly different [so that] the valence of the vertices were to be all even, then there would be a path of the desired kind. (Pincock 2007: 260)

According to Pincock's account, then, explanations like *Bridges* explain by demonstrating that so long as the system in question has the relevant higher-level properties (so long as it "still [has] the structure of the same graph"), the explanandum will obtain. For this reason, Pincock refers to the explanations as abstract explanations. In Pincock 2012, he suggests that he takes the same to be true of the *Cicadas* explanation: he mentions that the "specific genetic coding responsible for the actions of the cicada or the bees [referring to the extra-mathematical explanation of the fact that honeybees' honeycombs are hexagonal] are not relevant to the sort of explanation we have offered" (Pincock 2012: 210).[76]

So, coming to something like a reconstructed statement of the abstract dependence account, (abstract) extra-mathematical explanations function by using information about abstract dependence to demonstrate that the explanandum obtaining *is* explained by the fact that the target is appropriately linked (where being appropriately linked can be achieved by different relations) to an abstract entity and is therefore *not* explained by any of the microphysical details of the system in which the explanandum located. The abstract dependence account, on my reconstruction that draws on Pincock's distributed remarks, has a positive component (the explanation works by giving information about abstract dependence) and a negative component (the explanation does not work by appealing to microphysical details of the system). An example: the explanandum in *Bridges*, on the abstract dependence account, is fully explained by the fact that the physical bridge system is appropriately linked (in this case, is an instance of, or instantiates) to the graph (the abstract entity) and is therefore not

[76] It is true that the notion of leaving out microphysical detail drops out of the discussion in Pincock 2015: nevertheless, Pincock clearly considers *Bridges* to be an example of the kind of explanation he is discussing in Pincock 2015 and does not walk back his earlier remarks regarding the case. I take it Pincock 2015 to be appealing to a different case study to draw out *another* aspect of abstract (or extra-mathematical) explanation – the role played by abstract dependence.

explained by any of the facts about the bridge system that are not entailed by it being appropriately linked: for example, it being made of stone rather than gold.

It is worth briefly comparing my reconstruction of Pincock's account with one of the few others in the literature. In a forthcoming paper, Jansson and Saatsi understand Pincock's account as locating explanatory information as information about *in*dependence (Jansson & Saatsi forthcoming: 2), contrasting it with approaches to explanation that emphasise dependence.[77] I don't think this can quite be right. As I attempted to unpick above, Pincock stresses at various points in his discussion that the explanandum *depending on* the entity invoked is crucial: it is the presence of abstract dependence of the target system on an abstract entity that seems to distinguish such explanations, for Pincock. This constituted the positive component of the account.

There is always the risk, of course, that this is a terminological disagreement (especially given the close conceptual link between dependence and independence (which Jansson & Saatsi also stress)). Perhaps one provides information about independence *by* providing information about abstract dependence. However, it is more faithful to what Pincock says to take his insight to be that this special class of explanations is distinguished from ordinary causal explanations *not* because they appeal to information about independence rather than dependence but because the kind of dependence doing explanatory work is a *different* kind of dependence. Whilst in causal cases, physical dependence (construed causally) is doing the explaining, in these cases a special kind of *abstract* dependence can do explanatory work: what explains is not dependence of the explanandum on other parts of the world, but of the target system abstractly depending on an abstract entity. As suggested above, abstract dependence might be cashed out using different metaphysical notions: the idea being that the target might abstractly depend on the abstract entity *because* it is an instance of it, rather than abstract dependence being *identified with* instantiation. This makes better sense of Pinock's tentative discussion of a sort of general theory of explanation that appeals to different kinds of dependence relations. One might wonder about the anti-exceptionalist credentials the abstract dependence account, given that (like other existing theories) it gives a crucial role to dependence. Pincock, too, considers this question: it makes more sense, however, to bracket this interesting interpretive question until chapter 4.

[77] I discuss the positive proposal in Jansson & Saatsi forthcoming in the next chapter.

3.5.1. The cicada variation objection

Somewhat awkwardly, the most persuasive reasons for not endorsing the abstract dependence account concern Pincock's faulty reasons for *not* subsuming the account under Woodward's account of explanation, which also appeals to dependence. This will have to wait until after §4.2, where this account is set out. There is, also, of course a very easy response to give to Pincock: that of maintaining that it's *just not clear* what abstract dependence amounts to and that, until this point is clarified, it's difficult to assess both the plausibility of Pincock's account and whether or not it captures what's going on in the case studies. Pincock mentions ontological dependence as an example of a kind of non-physical dependence (Pincock 2015: 878) but does not *identify* abstract dependence with ontological dependence or any other kind of metaphysical relation. I don't know what abstract dependence is, and therefore do not know how plausible Pincock's account is.

As tempting as it is to give this response, an orthogonal objection to the abstract dependence account can be had using only using resources that Pincock provides in an earlier discussion, and so therefore not involving resort to footstamping about not understanding what Pincock means by 'abstract dependence'. I will set out this objection here. It is important to stress, from the get-go, that in a forthcoming commentary Jansson & Saatsi have separately offered an objection (aimed at accounts that (they claim) appeal to independence rather than dependence (see above)) along these same lines, appealing to versions of *Bridges* with different explanatory strengths. I take mine to be distinguished in that it has greater dialectical force when aimed at the abstract dependence account, given that it appeals to earlier work by the proponent of that account.

Recall from the previous chapter that Pincock introduces a variation of *Cicadas* (Pincock 2012: 211), for the purposes of arguing that *Cicadas* fails to meet his *Sensitivity* condition on IBE.[78] The weaker variation of the explanation appeals to a different number-theoretic claim. Instead of the explanatory number-theoretic claim being that prime periods minimise intersection, the variation of the argument appeals to the true generalisation that prime periods up to 100 years minimise intersection. There is existing disagreement in the literature as to which explanation is superior: the one Baker abstracts from the scientific literature, appealing to the general theorem, or the one that Pincock discusses, appealing to the

[78] See §2.6 for a discussion of this requirement.

restricted theorem. In response to the argument (for the purposes of which Pincock originally deploys this weaker variant), Baker provides some reasons for thinking that the stronger version of the explanation is superior. He says:

> Among other things, [the weaker theorem] is less general, it has less predictive power, it supports fewer counterfactuals, the precise restriction on the domain has no independent motivation, and it is less syntactically simple. (Baker 2015: 698)

As with Baker's diffuse anti-exceptionalism, his aims here are orthogonal to the aims of this chapter: Baker briefly lists these purported weaknesses of Pincock's alternate argument in order to side-step Pincock's objection. It is worthwhile unpacking this claim, though. There is reason to think that the first two strengths of Pincock's alternate explanation (and, correspondingly, the two strengths of the original *Cicadas*), that it is less general, less predictively powerful and supports fewer counterfactuals, are importantly connected. Indeed, the argument having less predictive power *just is* the argument supporting fewer counterfactuals: predictions about what life-cycle length the cicada would have if the ecological constraints were such that their life-cycles must fall in the range 101-113 (for example) can be made by the original version of *Cicadas* precisely because it provides an answer to this counterfactual inquiry. Furthermore, the fact that the original version of *Cicadas* is predictively more powerful, in virtue of supporting more counterfactuals, is grounded by the fact that the number-theoretic theorem is more general: *because* it makes a claim about prime periods in general, and not just a narrower subset, it can support more counterfactuals.[79]

It is important to note, however, that Pincock also holds that the explanations differ, but says we ought to prefer the weaker version of the argument, saying that:

> [R]easonable rules for the use of IBE suggest that, other things being equal, the explanation that employs the weaker claim is superior to the explanation employing the stronger claim. (Pincock 2012: 212)

[79] It is less clear that the last two purported benefits of the original *Cicadas* explanation are genuine benefits. The fact that the weaker version of the explanation lacks independent motivation does not make the explanation *worse*, unless the lack of motivation has any ramifications for the power of the explanation. Similarly, the fact that the explanation is less syntactically simple seems to make the same equivocation between various kinds of simplicity that Melia observed (Melia 2000: 473).

However, this is to confuse two different meanings of 'superior'. One concerns the explanation's strength and the other is concerned with epistemic humility: the idea being that when offering explanations, we ought to select the explanation that manages to successfully explain the phenomenon with the fewest commitments (for a useful discussion of when explanatory virtues might count as pragmatic, see Ylikoski & Kuorikoski 2010). An explanation might be a more powerful explanation with it still being the case that it is a mistake to accept it, if doing so is very epistemically risky. Baker's claim, I take it, is *both* that the original version of *Cicadas* is more powerful in the first sense and, also, that it is not epistemically risky to accept it.

I won't discuss this question any further here: although I think there is reason to endorse Baker's line regarding the two versions of *Cicadas,* what is most important for the purposes of critiquing the abstract dependence account is that the two versions are judged to differ in their explanatory character. Pincock's account is unable to vindicate this judgement: the central problem is just that from the little that Pincock says about abstract dependence, it seems as though it does not come in degrees, but explanatory strength seems to: recall that, at least in some cases, abstract dependence was had because the target is an instance of the abstract entity. Even admitting that the kind of abstract dependence that Pincock is interested in does come in degrees does not seem to help. We seemingly judge that the two cicada explanations differ in explanatory strength, yet they do not differ when it comes to the extent to which they are connected in the right way to an abstract entity (I take it, in this case, the natural number structure) *and* the extent to which the microphysical details play a role. Pincock could, of course, merely bite this bullet and maintain that the two explanations *are* identical in their degree of explanatory power and that appearances otherwise ought to be explained by gesturing towards pragmatic concerns associated with the act of explaining (see, again, Ylikoski & Kuorikoski 2010) – but this is in tension both what he has said earlier and, I suggest, the most natural understanding of the two explanations.

3.5.2. The abundance of abstract explanations

Recall the reconstruction of the abstract dependence account that I gave above: (abstract) extra-mathematical explanations function by using information about abstract dependence to demonstrate that the explanandum obtaining *is* explained by the fact that the target is appropriately linked (where being appropriately linked can be achieved by different relations) to an abstract entity and is therefore *not* explained by any of the microphysical details of the system in which the explanandum located. The worries raised above concern the first component, the notion of abstract dependence. During his discussion of causal and extra-mathematical explanation, Lange makes a remark that suggests an additional worry about Pincock's account, about the second, negative component: the claim that because the explanandum is explained by a fact about abstract dependence, it is not explained by microphysical details. (Repackaging Lange's remark as an objection to the negative component of the abstract dependence account does not turn on accepting any of the details of Lange's positive proposal).

Lange makes the (near-platitudinous) claim that "plenty of explanations abstract from petty causal influences, emphasizing mathematical structure instead, but are nevertheless causal rather than distinctively mathematical explanations" (Lange 2016b: 34) and referring to Batterman's claim that the relevant explanations involve "throwing away of various causal and physical details" (Batterman 2010: 3), says that "many causal explanations do that too – including explanations that appeal to one trait's having greater fitness than another (abstracting away from the detailed histories of individual mating, reproduction and predation events) [and] explanations that appeal to a peg's roundness and a hole's squareness (abstracting away from the particular intermolecular forces at work)" (Lange 2013a: 506).

Although Pincock discusses Lange's earlier presentation of the constraint account (Lange 2013a), he does not indicate whether or not he thinks the above observation(s) are problematic for the abstract dependence account. Yet, it seems clear that they are: if the sorts of explanations that Lange discusses count as abstract explanations, then it seems both that such explanations are abundant (suggesting that the account does not successfully capture the narrow class of extra-mathematical explanations) but, crucially, that they are not different in kind from regular explanations. This can be brought out by briefly discussing an example that Woodward and Hitchcock use to introduce their causal account of explanation:

> *Wire*
>
> What is being explained is "the magnitude of the electric field created by a long, straight wire with a positive charge uniformly distributed along its length" (Woodward & Hitchcock 2003: 4). Coulomb's law "can be used to tell us how the electric field would differ if the charge density of the wire were increased, or if the wire twisted into a circle or a solenoid. In this way, [the law] shows us that certain factors, such as charge density and geometrical

> configuration of the conductor, make a systematic difference to intensity and direction of the field." (*ibid*)

When coupled with Woodward and Hitchcock's interventionist account of causation (see Woodward 2003; Woodward & Hitchcock 2003 for details), *Wire* comes out as being causal (*cf.* Saatsi & Pexton 2013). (Even though Lange, above, makes the point that an explanation being abstract in the sense that Pincock discusses doesn't seem inconsistent with the explanation being causal, this is not to say that he endorses the interventionist account of causation). Nevertheless, it leaves out any of the microphysical information about the wire and as such can be used as an explanation for any physical system has the properties described in the explanans. It is unclear how this case differs from *Bridges*, at least in as much as it abstracts away from microphysical information about the system under consideration. Why does *Wire* not constitute an abstract explanation, with the explanatory target counting as an instance of an abstract entity that has amongst its instances all of the wires with the properties described? It is not clear that the abstract dependence account has the resources to explain why *Wire* does not count as such an explanation. At a first pass, one might press that this isn't a problem for Pincock's view *qua* account of extra-mathematical explanation: perhaps abstract explanations *just are* abundant. It seems, however, that a consequence is that Pincock's account cannot distinguish the special class of explanations that of interest from regular explanations that use mathematics. It is this, especially, that suggests looking elsewhere.[80] With both Lange and Pincock's accounts, the fact that explanations by constraint or abstract explanations are abundant doesn't seem *by itself* to be an objection: but in both cases, in different ways, this had consequences that rendered the accounts problematic (in the constraint case, that non-explanations are rendered explanatory and in the abstract dependence case, that extra-mathematical explanations are not properly distinguished from other explanations).

The situation, then, is as follows. Much of what Pincock says about the individual cases seems right-headed: what seems to be mistaken is his claim that these insights cannot be captured by causal accounts of explanation. At heart, this is also what is wrong with Lange's account: he is not *wrong* that extra-mathematical explanations sometimes admit to a degree

[80] Consider that *Pendulum*, too, seems to meet the requirements of Pincock's notion of abstract dependence. Yet this was introduced as the kind of case where it seems clear that the explanation is a regular explanation that uses mathematics, and not an extra-mathematical explanation.

of necessity stronger than many other explanations, but is too quick in thinking that this helpful insight cannot be captured by existing accounts of explanation.

3.6. Conclusion

In this chapter I considered four recent accounts of extra-mathematical explanation: some of these are anti-exceptionalist whilst some are exceptionalist, and some are offered explicitly in order to boost the prospects of the enhanced indispensability argument whilst others are offered merely to shed light on this interesting kind of explanation.

Some of the problems with the accounts turned on their particular details. There are a couple of recurring themes, however. The first is that the line between extra-mathematical explanation and causal explanation seems less straightforward than appearances suggest. The second is that the role of dependence popped its head up at various points: problems with Baker's entailment account and Lange's constraint account can plausibly be resolved by appealing to notions of dependence whilst some of the uncertainties surrounding Pincock's account arose from the notion of *abstract* dependence. Turning to the current literature on scientific explanation, there is a long tradition of explicating explanation by appealing both to notions of causation and to notions of dependence (of a certain kind). The lesson of this chapter, then, is that it is worthwhile to see if these resources can shed light on extra-mathematical explanation. This is the task of the next chapter.

Chapter 4

The modal account of extra-mathematical explanation

In the previous chapter I considered some recent accounts of extra-mathematical explanation and argued that there are reasons to look elsewhere. In this chapter, I motivate and endorse an alternate account of extra-mathematical explanation. According to the modal account of extra-mathematical explanation, such explanations function by demonstrating the ways in which the explanandum depends on the explanans facts and, in doing so, allow a user of the explanation to answer *what-if-things-had-been-different* questions about the explanandum. Extra-mathematical explanations have more in common with causal explanations (at least, as understood by those working in the interventionist tradition) than usually thought. Accordingly, a form of anti-exceptionalism about extra-mathematical explanation is vindicated.

In the first section I return to a thought briefly noted in discussing Baker's entailment account: namely, that the reasons he gives for eschewing attempting to extend a causal account to the extra-mathematical case are unconvincing. I work through some ways of trying to make the judgement that causal accounts are unsuited for our task more precise and suggest that, after they are fleshed out, the worries reveal themselves to be unpersuasive. In the second section I offer some *prima facie* reasons in favour of appealing to Woodward's interventionist account of causal explanation, over other theories of causal explanation. I then make salient the relevant features of Woodward's account. In the third section I make the case that a Woodward-style account can shed light on extra-mathematical explanation in what I take to be the most appropriate way: by working through some of the case studies set out at the end of chapter 2. The basic idea behind this extension of the account is that the mathematical fact is analogous to the invariant generalization that features in Woodward-style treatments of paradigm causal cases. Until reasonably recently, the only discussion in the literature regarding the plausibility of a Woodward-style account of extra-mathematical explanation was negative (see, for example, the discussion in §4.5). However, there are also some recent and forthcoming positive proposals along these lines, which I have incorporated into the discussion in this chapter. In order to contribute to this promising emerging line of thought, in the fourth section I draw on forthcoming work to adjudicate whether or not extending a counterfactual account of explanation to the extra-mathematical case involves appealing to counterpossibles resulting from perturbations of mathematical facts. I locate the source of this implicit disagreement and argue (in line with Jansson and Saatsi and contra Baron, Colyvan and Ripley, and Chirimuuta) that extending the account does not involve counterpossibles. In doing so, I alleviate a potential burden on the modal account, that of giving an account of the semantics of counterpossibles. In the fifth section I briefly discuss and reject the aforementioned objection(s) to approaches like that of this chapter, owing to Pincock. In the sixth section I raise a pressing outstanding question for the modal account of extra-mathematical explanation, as it stands. I motivate the claim that an explanation must be given of *why* a mathematical fact allows access to modal information about the explanandum in the same way that an invariant generalization does in cases of regular causal explanation: an answer to what I call the *in virtue of* question. I tentatively suggest that the structural nature of extra-mathematical explanations can shed light on this feature and provide an answer to the *in virtue of* question. In section seven I provide an overview of the benefits of the modal account and compare it to its rivals, discussed in chapter 3. In section eight I discuss whether or not the success of the modal account reveals that extra-mathematical explanations are causal explanations, after all. In section nine I draw some conclusions from this chapter.

4.1. Fretting in the literature

Both in the literature and in conversation, philosophers take it for granted that there is *something* about extra-mathematical explanations that make the project of this chapter a non-starter. Although we seemingly know very little about extra-mathematical explanation, one thing that we apparently *do* know is that causal theories of explanation won't be a helpful tool in shedding light on this special class of explanations. In this section I set out four different ways of motivating and articulating this worry and argue that none amount to reasons for dismissing attempts to extend causal accounts of explanation to the extra-mathematical case. These worries are distinct from objections to the *particular* account of extra-mathematical explanation offered here. The worries responded to in this section are more fundamental, in that they suggest that there is something wrongheaded about *any* attempt to accommodate the extra-mathematical cases using a causal account of explanation.

The first version of the worry states that extra-mathematical explanations do not provide causal-mechanical information and therefore are not ripe for accommodation by a causal account of explanation. Batterman seems to think that such explanations cannot be understood as broadly causal in virtue of the fact that causal explanations contain information about the "causal-mechanical […] workings of the mechanisms" that bring about the explanandum (Batterman 2000: 28) (see §3.5.2). Rice says something like this in describing optimality models:

> [T]hese idealizations entail that optimality models usually provide little, if any, accurate information about the actual causes, or causal mechanisms, within the model's target system (s). In the end, the highly idealized optimality model represents mathematical relationships between constraints, tradeoffs, and the system's equilibrium point that do not mirror any causal relationships (or processes) in the target system. (Rice 2015: 600)

Is something like this true of the extra-mathematical explanations considered here? An inspection of some extra-mathematical explanations reveals that it is plainly *false* that extra-mathematical explanations do not provide information about causal processes.[81] Referring to some of the case studies brings this out. A crucial component of any optimality explanation is the presence of evolutionary pressures. Having prime periods is only adaptively beneficial because at some point in the evolutionary history of the cicada there were periodical predators. Accordingly, very many extra-mathematical explanations provide some information about the explanandum's causes, and this point has already been noted (see, for example, Lange 2016b: 22 – 23).

Here's a second way of trying to make this worry precise. Recall that Baker says that "according to the causal account, explaining a phenomenon involves giving a description of its various causes. Clearly this account is incompatible with the existence of *any* genuine mathematical explanations, since mathematical objects (if they exist) are acausal" (Baker 2005: 454). Baker's suggestion here that extending causal accounts will fail because an extra-

[81] There is reason to think that this is not in tension with much of what Rice says in his discussion. It is important to distinguish between an explanation of the fact that it is optimal for the cicadas to have prime-numbered life-cycles, given certain other conditions hold (for example) and an explanation of the fact that cicadas evolved to have prime-numbered life-cycle. *That* it is optimal to do so obviously features in this latter explanation but without endorsing a view about the transitivity of explanation (see Hicks & van Elswyk 2015; Lange 2013b; Lower 2012) it is not straightforward to demonstrate that an explanation of the fact that prime-numbered life-cycles are optimal is also an explanation of the fact that cicadas evolved to have such life-cycle, an explanation which appeals to *the fact that* such life-cycles are optimal.

mathematical explanation is one in which *the existence of a mathematical object* is doing the explaining, and mathematical objects are acausal.[82] This is consistent with mathematical explanations including some causal information. It, however, seems to disregard the possibility that a piece of mathematics can do explanatory work without it being the case that the existence of a mathematical object does explanatory work. Talking about a case study, Pincock says that "an easy way to see that causal dependence is not involved would be by emphasizing the highly mathematical character of [his case study]. As nobody thinks that causal relations obtain in pure mathematics, we have a non-causal explanation" (Pincock 2015: 867). I agree with Pincock that there are no causal relations in pure mathematics and that if mathematical objects exist, then they do not *cause* the explananda of extra-mathematical explanations. But, as Pincock seems to acknowledge at other points in his discussion (see §3.5 and §4.5), contemporary causal accounts of explanation do more than merely state that the objects appealed to in the explanans stand in causal relations to the objects featuring in explanandum. This will be brought into sharper relief when setting out the modal account of explanation in detail below. This purported reason for not attempting to extend causal accounts is perhaps a result of the fact that debate about extra-mathematical explanation stems from the enhanced indispensability argument, which focuses on mathematical objects and their possible existence.

A similar worry might be motivated by the thought that a unified account should be given of intra-mathematical explanation and extra-mathematical explanation. Intra-mathematical explanations are mathematical explanations of *mathematical* facts: a natural thought is that although every distinct proof of a theorem demonstrates *that* the theorem is derivable, not all theorems *explain why* the theorem obtains (Mancosu 2008; Lange 2014).[83] Combined with an assumption that a causal account of explanation *definitely* won't be of use in the intra-mathematical case (Colyvan forthcoming: 3), the claim that a unified account should be given of all kinds of mathematical explanation leads naturally to the conclusion that causal accounts should not be appealed to in understanding the cases currently under inspection. I won't say much here about whether or not extending causal accounts of explanation to intra-

[82] It is commonplace to assume that mathematical objects, if they exist, are acausal. I will assume this here, although for an interpretation of mathematics that appeals only to the part/whole relation, concrete objects and some assumptions about the mereological structure of the world, see Lewis 1991 and Lewis 1993.

[83] This is not to say that all explanation within mathematics obtains between a theorem and a particular proof of that theorem: many of the interesting examples provided by Maddy of introducing a new axiom on the basis that it unifies various previously disparate mathematical phenomena (Maddy 1997: 208-219) seems, on the face of it, an act of explanaining by unifying, but what is doing the explanatory work here is an axiom.

mathematical explanation is a viable project: rather, the demand that one accommodate extra-mathematical explanation and intra-mathematical explanation using the same account is under-motivated. If it is taken for granted that intra-mathematical explanation is not going to be accommodated by an account of causal explanation, then one is either going to have to accommodate *extra*-mathematical explanation into one's account of scientific explanation *or* into one's account of mathematical explanation. I don't think any distinctive features of the case studies pull decisively in either direction: the fact that a mathematical fact is playing a role in the explanation might be taken to pull in the latter direction, but the fact that *the fact being explained* is a non-mathematical fact pulls in the former direction. We should proceed by developing accounts and seeing how they compare. The desire for a unified account might also stem from the thought that in cases where a theorem is appealed to in a scientific explanation, it is the explanation of the proof that counts, too, as the explanation in the scientific context. Although Colyvan has once argued that this is the case (Colyvan 2001), he has since suggested that there is "reason to be suspicious of this claim" (Baker & Colyvan 2009: 326), following Baker's suggestion that there are theorems that play an explanatory role in scientific contexts for which there is not (at present, at least) an explanatory proof (Baker 2012). It seems that defending a view along the lines of Colyvan's earlier view requires defending some strong theses about the transitivity of explanation that, in this context at least, have not been defended (for details on this debate see Hicks & van Elswyk 2015; Lange 2013b; Lower 2012).

Here's a final, and most charitable, version of the objection. This final version of the objection says that *what is doing the explaining* in the explanations is not information about causes. On this line, even if an extra-mathematical explanation provides some causal information (like the existence of predators in the cicada case), and even if it is not the existence of a mathematical object that is doing the explaining, *whatever* the source is of explanatory power, it is not information about causes. The cicada explanation could have added to it more fine-grained information about the causal process involving the adaptive pressures, but this would not make it a better explanation. In some sense, the particular causal-mechanical process is irrelevant to the evolution of the particular behaviours or attributes that are being explained in optimality explanations. The fact that the amount of causal-mechanical information provided has no ramifications for the explanatory power of the explanation reveals, on this line of thinking, that it is not the provision of causal-mechanical information that is doing explanatory work (even if some causal-mechanical information is present in the explanation and even if it is not the existence of a mathematical object that is doing the explaining). This is what Lange has in mind when he says that "this [extra-mathematical] explanation is non-causal because it does not *work by* describing the outcome's causes or, more broadly, the world's network of causal relations" (Lange 2013a: 493) (emphasis added), drawing on Lipton's remark that "causal explanations are explanatory *because* they are causal" (Lipton 2004b: 32) (emphasis in the original).[84]

This last version of the objection is, clearly, the most charitable way of understanding the worry.[85] It allows for the fact that extra-mathematical explanations contain causal information whilst still maintaining that any accounts of explanation that involve causal explanation *being explanatory* are non-starters for the extra-mathematical case. Yet, there is still something not quite right here. There is something wrongheaded about the claim that, even if extra-mathematical explanations cite information about causes, this information is not the source of explanatory power. The purported facts about the evolutionary history of the cicadas, and the evolutionary history of the marine mammals, clearly play *some* explanatory role in the relevant explanations. A simple replacement test seems to bring this out.[86] Absent the fact about the evolutionary history of the cicadas, the explanation seems to be no explanation at all: if there were no periodical predators, then it would remain a mystery as to why the cicadas evolved to have prime life-cycles, even accepting the truth of the number-theoretic theorem. Having a prime life-cycle would confer no evolutionary benefit on the cicadas in this case.[87]

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[84] Relatedly, one might worry that if causal information and causal explanation are tightly related to counterfactuals then the necessity of mathematical facts makes causal resources useless for understanding extra-mathematical explanation. I will return to this worry after setting out an account of extra-mathematical explanation that draws on the resources of a leading account of causal explanation.

[85] The purpose of discussing and rejecting the less charitable readings of the worries is that, as evidenced above, these readings are suggested in the literature and are also encountered 'in the field'.

[86] Appealing to a replacement test to work out whether or not a fact is genuinely part of an explanation is also done by Pincock. Pincock's test is more stringent than the heuristic appealed to above: see Pincock 2012: 204-205 for details. Of course, such a test can (at most) tell us *that* the fact is part of the explanation and cannot tell us *in virtue of what* the fact is explanatory.

[87] This worry is complicated by the fact that at least one interlocutor in the debate *denies* that the standard optimality explanations are cases of extra-mathematical explanation. Lange claims that, along with the other optimality examples, *Cicadas* is "just an ordinary causal explanation" (Lange 2013a: 499). Talking about another optimality explanation (the honeybee case introduced by Lyon (Lyon 2012)), Lange says that "this explanation works by describing the relevant features of the selection pressures that have historically been felt by honeybees, so it is an ordinary, causal explanation, not distinctively mathematical" (Lange 2013a: 499-500). He claims that the fact that has a genuinely mathematical explanation is that "in connection with predators having periodic life-cycles, cicadas with prime periods tend to suffer less from predation than cicadas with composite periods do" (*ibid*). (This corresponds to an explanation *of the fact that* it is optimal for the species to have this or that trait – see fn. 81). It is less than clear what Lange's reasons are for thinking that the original *Cicadas* explanation does not count as a genuinely extra-mathematical explanation – on this line, it is a regular causal explanation that, nevertheless, necessarily appeals to a number-theoretic fact! (Lange says that it "uses a bit of mathematics in

This fact generates an additional condition on a successful account of extra-mathematical explanation. An account must accommodate the fact that *both* the causal information in the explanation *and* the mathematical information in the explanation play a role. A successful account of extra-mathematical explanation cannot render all non-mathematical facts cited in the explanation as non-explanatory. Even the simplest examples of extra-mathematical explanation involve some non-mathematical facts doing some explaining: the facts about the bridge configuration in *Bridges* is a physical fact about the number of bridges leading to and from each island, the facts about the number of children had by the parent in Lange's strawberries case (and that cutting strawberries is forbidden by stipulation) and so on. [88]

Given that it is a requirement that an account of extra-mathematical explanation tells us how both the mathematical *and* non-mathematical facts do explanatory work (in turn making sure not to render the non-mathematical facts as non-explanatory), and given that the non-mathematical facts doing the explaining in many extra-mathematical explanations are facts about causation, this further motivates the project of attempting to extend causal accounts of explanation to the extra-mathematical case. As I will argue in the next two sections, a modal account of extra-mathematical explanation can render both the mathematical and causal information explanatory. Indeed, it can do so in a way that is unifying whilst still maintaining a distinction between the explanatory facts of two kinds: both the mathematical

describing the explanandum's causal history" but this doesn't seem quite right – the number-theoretic theorem is stating a mathematical fact, not (at least, on the surface) describing the explanandum's causal history). Lange, elsewhere, admits the existence of explanations in which *both* causal and mathematical facts do explanatory work. In a separate discussion on transitivity and self-explanation, Lange discusses an explanation (involving the losing of a bet and the pigeonhole principle) that "is supported not just by causal relations and not just by distinctively mathematical relations, but rather the two in combination" (Lange 2016a: 9). Lange's explanandum with (what is, to Lange), the *genuinely* mathematical explanation ("in connection with predators having periodic life-cycles, cicadas with prime periods tend to suffer less from predation than cicadas with composite periods do") lacks any causal information *only because* this has been built into the why-question, as it were. The existence of periodical predators is built into the phrasing of the explanandum ("in connection with predators having periodic life-cycles"). We should understand *Cicadas*, as canonically stated (e.g., as in Baker 2005 & Baker 2009), is an explanation where causal and mathematical information is involved in the explanation: Lange's alternate explanation is one where the why-question has been set up such that *only* a mathematical fact is involved in the explanation but Lange does not motivate the view that a distinctively mathematical explanation must be one where *only* a mathematical fact plays a role in the explanation rather than, more weakly, just one where a mathematical fact is involved in the explanation.

[88] Much of this information is, of course, normally expressed mathematically but can easily be given a non-mathematical rendering in first-order logic with identity. (One might worry that if the bridge information is not given in terms of the number of bridges but in terms of oddness and evenness, this is a barrier to expressing the fact about the bridge non-mathematically, but this is not the case, as these concepts too are susceptible to a non-mathematical formulation (as Tallant demonstrates in his proposed nominalisation of the concept of *primeness* (see Tallant 2013 for details)).

and causal information play crucial roles in allowing a user access to counterfactual information.

4.2. Woodward's account of causal explanation

In this short section I justify focusing on Woodward's account of explanation in carrying out the task of attempting to extend accounts of causal explanation to the extra-mathematical case, and then set out the core details of Woodward's account, making salient those aspects relevant for the purposes of this chapter.

4.2.1. Why Woodward?

The path towards extending Woodward's account of explanation to the extra-mathematical cases was cleared by the dispelling of *prima facie* worries about extending a causal account of explanation in §4.1. However, Woodward's is not the only account of causal explanation on the market and there are, of course, extant worries about the account (for example, worries that it cannot respect our judgements about the relationship between higher- and lower-level explanations (Franklin-Hall 2016; Weslake 2010) and even that it gets our judgements about simple cases wrong (Skow ms)). Indeed, it is not even as if Woodward offers the only counterfactual treatment of causation or causal explanation (see, non-exhaustively, Hitchcock 2007; Lewis 1986; Schaffer 2005; Yablo 2004). Why, then, attempt to extend Woodward's account rather than some *other* account of explanation, such as Strevens' Kairetic account, which focuses on difference-making?[89]

Much of this question turns on questions internal to the causal explanation debate: reasons for preferring Woodward's account to Strevens' *qua* account of causal explanation. One might be moved by the fact that Woodward's account seemingly accommodates counterexamples that plague other accounts, that aspects of the account have been accepted and accommodated by those working in the mechanisms literature (Craver 2007) and by the fact that the account makes do without appealing to a metaphysically substantive (or, as

[89] Saatsi (Saatsi 2016b) discusses extra-mathematical explanation in relation to the Kairetic account and Strevens himself discusses the relationship between causal and mathematical explanation (Strevens forthcoming). I hope to engage with this latter piece in future work: even though the projects of making sense of extra-mathematical explanation using a Woodwardian framework and using Strevens' framework are distinct, given that they are both in the business of using a causal theory of explanation to accommodate such cases, there are likely to be insights and strategies common between the two.

Woodward has it, 'reductionist') account of causation (Woodward 2003: 20-21). If one of the aims of extending Woodward's account of explanation to the extra-mathematical case is to vindicate anti-exceptionalism, it would be an awkward state of affairs if a Woodward-inspired account of extra-mathematical explanation ended up being successful but Woodward's account of *causal* explanation ended up being inadequate compared to the Kairetic account, for example.[90] It hopefully goes without saying that it is beyond the current scope to evaluate the two accounts *qua* accounts of causal explanation, let alone compare them to other accounts of causal explanation.

Here, then, are three *prima facie* reasons for attempting to extend Woodward's account of explanation rather than others – reasons that do not depend on the success of Woodward's account as an account of explanation *qua* causation. First, recall that the notion of dependence cropped up at various points in the discussion of rival accounts in the previous chapter. Some of the problems of the entailment account and the constraint account involved generating the wrong judgements about different variations of the cases, and there were outstanding questions about how Pincock's notion of abstract dependence relates to other kinds of dependence appealed to in existing accounts of explanation.[91] This, in turn, leads the way for attempting to extend Woodward's account in particular, which gives a key role to notions of dependence in a sense that is distinctive when compared to similar accounts. Second, many of the examples of extra-mathematical explanation are drawn from the biology literature and interventionist accounts of explanation (including mechanistic accounts of explanation that draw upon an interventionist notion of causation) are often taken to be good candidate theories for shedding light on these explanations. Third, there is existing recent work on extending Woodward's account to non-mathematical non-causal explanations (Bokulich 2011; Reutlinger 2014; Rice 2013; Saatsi & Pexton 2013) – appealing to Woodwardian resources to shed light on extra-mathematical explanation, then, can be positioned in a current research program. So, there are reasons for preferring Woodward's account over others: it does well in an internal comparison of accounts of explanation, it appeals to the notion of dependence in a way that may help avoid the problems of rival accounts and coheres both with the literatures on explanation in the biological sciences and on non-causal scientific explanation.

[90] This, of course, wouldn't *tell against* a Woodwardian account of extra-mathematical explanation.
[91] See fn.114.

These, of course, are not decisive reasons for appealing to Woodward's account of explanation rather than another current account of explanation. If a rival anti-exceptionalist account of extra-mathematical explanation can be offered, then this account will have to be compared with the anti-exceptionalist modal account offered in this chapter. Hopefully, however, it is sufficient justification to motivate the project of this chapter.

4.2.2. Woodward's account of causal explanation

In this section I set out the core commitments of Woodward's account of causal explanation: this will allow an analysis of how a Woodward-style treatment of extra-mathematical explanation is both similar and dissimilar to the analogous treatment of causal explanation.

According to the counterfactual account of explanation, explanation "is a matter of exhibiting systematic patterns of counterfactual dependence" (Woodward 2003: 191). In particular, the relevant counterfactual dependencies are those that facilitate the answering of questions concerning how the explanandum would have been different if the explanans had been different in certain, particular ways. In particular: if some particular intervention had been carried out on an explanans variable, how would this have affected the explanandum variable? As Woodward says:

> [Explanations] locate their explananda within a space of alternative possibilities and show us how which of these alternatives is realised systematically depends on the conditions cited in the explanans. They do this by enabling us to see how, if these initial conditions had been different or had been changed in various ways, various of these alternative possibilities would have been realised instead. [Explanations] can be used to answer a range of counterfactual questions about the conditions under which their explananda would have been different. (Woodward 2003: 191)

Skow offers the following one sentence summary of the view:

> A body of fact F causally explains some event (or, equivalently, explains the fact that the explanandum variable takes a particular value) E iff F exhibits a systematic pattern of counterfactual dependence of E on other events (or, equivalently, exhibits a systematic pattern of counterfactual dependence of E on the explanans variables taking particular values). (Skow *ms*: 5)

It is clear, of course, that not all information about counterfactual dependency can count as explanatory: as Woodward says, "there seems to be a perfectly good sense in which the joint effects […] of a common cause […] are counterfactually dependent on one another, even though one cannot appeal to the occurrence of one effect to explain the other" (Woodward 1997: S29).[92] Rather, on Woodward's account of explanation, the counterfactuals that are explanatorily salient are those that take as their antecedent a description of an intervention. Woodward develops a technical notion of intervention, such that changes in the explanans variable that are a result of changes in the explanandum variable or a result of a different variable that effects the explanandum variable independently of the explanans variable are ruled out. To explain, then, is to provide information about counterfactual dependence relations (of the right type) between the explanandum (variable) and the explanans (variables). A helpful (though in ways that will become clear, occasionally misleading) way of understanding the notion of an intervention is as a manipulation on an explanan variable that could have physically been carried out, resulting in the explanandum variable taking a different value. This heuristic can, at least, see how the account offered avoids common cause counterfactual dependencies as counting as explanatory. It is nevertheless important to note that the relevant set of interventions are not those that we could, in fact, carry out. Woodward appeals to the notion of potential exploitability:

> What matters is not whether human beings can actually carry out manipulations on the magnitudes of X and Y but whether the relationship correctly describes how Y would change if a change in X were produced by a special sort of causal process that I call intervention (Woodward 2003: 374).

Woodward says that "I call a manipulation having the right sort of structure an *intervention*" (*ibid*: 28) (original emphasis). For our purposes, it is safe to take the manipulations of variables to include *all* changes in variables and for the interventions to be a subset of the manipulations – every intervention is a manipulation (because every intervention involves a changing of an explanan variable) but not every manipulation is an intervention (as to count as an intervention, a manipulation must meet some additional criteria – it must have

[92] Consider a perfectly accurate barometer: there will be counterfactual dependence of the barometric reading on the atmospheric pressure in much the same way as there will be counterfactual dependence of the atmospheric pressure on the barometric reading (*cf.* Salmon 1989: 46-50).

Woodward's "right kind of structure").[93] The relevant manipulations (those that count as possible interventions) for Woodward are not exhausted by those manipulations that we could in fact carry out but also do not contain *all* changes in explanans variables (for discussion see *ibid*: 127-133):

> I suggest that as long as there is some basis for assessing the truth of counterfactual claims concerning what would happen if various interventions were to occur, it doesn't matter that it may not be physically possible for those interventions to occur. (*ibid*: 130)

Whether or not a more precise characterisation must be given in order for Woodward's account, *qua* account of causal explanation, to be successful is not one I pursue here but return to briefly in §4.8 when discussing the causal nature of extra-mathematical explanations.

Another crucial component of Woodward's account to be made salient is that of an invariant generalization. The invariant generalization plays the role of telling us how changes of the explanans variable results in changes of the explanandum variable. Consider *Pendulum*, an explanation introduced at the end of chapter 2.[94] The pendulum law tells us how, if we were to intervene on an explanan variable, the explanandum variable would take a different value. What is it for a generalization to be invariant? For Woodward, invariance comes in degrees. Consider an example given by Woodward. Newton's law describes a relationship between certain variables: it tells us how these variables depend on each other. The generalization is invariant under a change of variables if, under that change, the values described by the generalization stand in the relationship described by the generalization. In Woodward's example, Newton's law of gravitation is an invariant under changes that do not involve strong gravitational fields or velocities comparable with light: once the variables take *these* values, the relationship that the variables stand in to each other is no longer as described by the generalization (*ibid*: 286).[95]

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93 It is unfortunate that both 'intervention' and 'manipulation' have human-focused rings to them and that sometimes in the literature they are treated interchangeably: the above hopefully accounts for how the terms are used in this thesis.

94 Whilst *Pendulum* has mathematical content, recall that it is not an example of a *mathematical* explanation in the sense relevant for this chapter (see §2.7).

95 *That* invariance is a matter of degree does not tell us *how* it ought to be measured. The plausibility of measuring invariance simply by appealing to the number of interventions that a given generalization is invariant under seems to turn on open questions about the density of the possibility space (the worry being that every generalization will end up being invariant under an infinite number of interventions, so long as we carve up the space of possibilities in the right way). Elsewhere, when Woodward talks about laws of nature, he says that:

I will briefly attend to two interpretative questions about the account. The first concerns what it is for a collection of facts to exhibit information about counterfactual dependence. It is clearly not the case that the body of fact that constitutes the explanation explicitly states all of the answers to the *w-questions,* even though allowing a user to answer such questions is the source of explanatory power. As Woodward says, explanations enable "us *to see how*, if these initial conditions had been different or had been changed in various ways, various of these alternative possibilities would have been realised instead" (*ibid*: 191) (emphasis mine). I think we should understand this roughly in terms of knowledge, or abilities, gained by a user of the explanation. Once a user of an explanation comes to know the information about counterfactual dependence, they gain the ability to answer *w-questions*: questions about what the explanandum variable would be if the explanans variables had changed as the result of an intervention.

A second question concerns why it is that the account described is an account of *causal* explanation, rather than of explanation *simpliciter*. This is in virtue of the fact that, for Woodward, causation is closely tied up with the notion of an intervention and, as set out above, the notion of an intervention is in turn used to narrow down the collection of counterfactuals. Woodward offers the following view on causation and intervention:

> A necessary and sufficient condition for X to be a (type-level) direct cause of Y with respect to variable set V is that there be a possible intervention on X that will change Y or the probability distribution of Y when one holds fixed at some value all other variables Zi in V. A necessary and sufficient condition for X to be a (type-level) contributing cause of Y with respect to variable set V is that (i) there be a directed path from X to Y such that each link in this path is a direct causal relationship; […] and that (ii) there be some intervention on X that will change Y when all other variables in V that are not on this path are fixed at some value. (*ibid*: 59)

"paradigmatic laws are simply generalizations with wide scope that are invariant under a large and important set of changes that can be given a theoretically perspicuous characterization" (*ibid*: 286). This suggests that invariance turns on the number of *important* interventions that a generalization is invariant under. This question needn't be settled here: what is important is just that invariant generalizations play their role in an explanation by giving the user of the explanation the ability to answer *w-questions* about the explanandum by describing how the values of the variables relate to one and other, and that their invariance is a matter of degree.

Taking explanation to centrally involve information about counterfactual dependence, and taking causation to be closely related to the notion of an ideal intervention, are logically distinct. As is noted by Woodward in passing (*ibid*: 221) and has been exploited by recent commentators (Bokulich 2011; Reutlinger 2014; Rice 2013; Saatsi & Pexton 2013), this leaves open the possibility for extending the account to kinds of non-causal explanation by taking as, in these cases, counting as relevant some counterfactuals that result from manipulations of explanans variables that do not (at least, not straightforwardly) result from an intervention.[96] It also means that Woodward's framework can be adopted even though one may have reservations about the interventionist analysis of causation. This is the insight that guides the extension of the account to the extra-mathematical case, given in the next section.

4.3. Towards a modal account of extra-mathematical explanation

As noted above, the account is presented by Woodward as an attempt to model causal explanation. This is reflected in Woodward's discussion of which class of counterfactuals is explanatorily relevant. The explanatorily relevant counterfactuals are those that contain information about the consequences of interventions. The explanatorily relevant counterfactuals are supported by contingent explanatorily generalisations, some of which are laws.[97] However, there is nothing in the counterfactual account to say that the explanatorily relevant counterfactuals must be supported by contingent generalisations: indeed, I'll suggest below that in the extra-mathematical cases the explanatorily relevant counterfactuals are supported by the mathematics used in the explanations. As noted by others, Woodward himself raises this possibility when he says:

> The common element in many forms of explanation, both causal and noncausal, is that they must answer what-if-things-had-been-different questions. When a theory tells us how Y would change under interventions on X, we have (or have the material for constructing) a causal explanation. When a theory or derivation answers a what-if-things-had-been-different question but we cannot interpret this as an answer to a question about what would happen under an intervention, we may have a noncausal explanation of some sort. (Woodward 2003: 221)

[96] I will return to this point in more detail in §4.8.

[97] Woodward is clear that even though in many cases the invariant generalization *will* be a law, its lawhood is not what enables it to play an explanatory role: the fact that it is invariant and accurately describes the world's patterns of dependence is what makes it explanatory. For details, see Woodward 2003: 286. (On some accounts of laws (e.g., Bird 2004), some of the explanatory generalizations will be necessary – nothing here turns on my assumption that they are contingent).

The best way to demonstrate the fruitfulness of the approach is to work through some of the examples set out at the end of chapter 2 and see how a Woodward-style approach can shed light on them. (In this section I set out the basic idea and in the next section appeal to recent and forthcoming proposals to introduce a bifurcation of the account). The idea is just that encapsulated in Woodward's quote above. The explanatory power of extra-mathematical explanations is located, as with causal explanations on Woodward's approach, in their ability to tell us what the explanandum depends on and in what ways: what aspects of the target system play a role in determining the explanandum obtaining and how the explanandum would be different, if these various aspects of the target system were different in various ways. The extra-mathematical extension functions by drawing an analogy between the role played by invariant generalizations in paradigm causal cases and the role played by the mathematical fact. In an extra-mathematical explanation, the mathematical fact (along with the other information in the explanation) takes us from values of the explanans variables to values of the explanandum variable.[98]

Start with the simple case. In Lange's strawberry case, the inability of the parent to evenly divide the strawberries amongst the children is explained by the mathematical fact that 23 is not evenly divisible by 3, and the non-mathematical facts that there are 3 children and 23 strawberries.[99] In this case, there are counterfactual dependence relations between the explanandum and the explanans that ground asymmetric changes in the explanans to changes in the explanandum. The explanation affords us knowledge that the number-of-strawberries variable has ramifications for the divisibility by three and, in turn, this has ramifications for the failure or success of the ability to evenly distribute the strawberries. That is: we know the answer to the w-question "what if the parent had 30 strawberries, rather than 23?". Crucially, this dependence of the explanandum variable on the explanans variable is asymmetric. Whether or not the parent can successfully share the strawberries amongst their children without cutting any strawberries counterfactually depends on the number of strawberries that the parent has, but the number of strawberries that the parent has does not

[98] Given that Lange's account also assigns a crucial role to modality, it is somewhat infelicitous to call the account here the modal account. However, given that Lange has recently noted that he thinks that extra-mathematical explanations belong to a wider class of explanations by constraint, the least inappropriate labelling of the views is the one used in this thesis.

[99] As explained above, the existence of paraphrases of these sentences in first-order logic with identity suggests that these are non-mathematical facts expressed mathematically. (For reasons familiar from §1.5, though, it is not a sufficient condition on a mathematical sentence being the mathematical expression of a non-mathematical fact that it is possible to express this content non-mathematically).

counterfactually depend on whether or not the parent is able to successfully share the strawberries.[100] The mathematical fact supports the counterfactuals in the same way that the generalisations do in the causal cases. This simple case additionally very clearly brings out how the mathematical facts in extra-mathematical cases and the invariant generalizations in the paradigm causal cases are very different kinds of facts, despite both playing similar roles in their respective explanations: there is no straight-forward sense in which the mathematical fact about 23's divisibility is *about* the strawberries in the same way that the pendulum law is *about* pendulums. I return to this point in §4.6.

Next, consider *Cicadas*. The explanatory power in this case can also plausibly be located in counterfactual dependence relations between the explanans (variables) and explanandum (variable). Baker in fact discusses two different explananda. The first explanandum is the fact that cicada life-cycle lengths are *prime*. Second are the more specific explanations of particular species' life-cycle lengths: explanations of the fact that the *Magicicada septendecim* has a life-cycle length of 17 years, for example. For the first kind of explanandum, the explanandum variable will take the value either 'prime' or 'not-prime'. For the second kind, the variable will take a value corresponding to the length of the life cycle measured in years – so, '17 years' is an example value. The explanans variables are things like the existence of periodic rival species and predators, the ecological constraints, and so on.

One of the explanans cites a fact that there are certain ecological constraints. The environment in which the cicadas exist is such that their life cycles must fall within a certain range (between 14 and 18 years). The possible values of this variable, then, are different life-cycle ranges, with '14 – 18' being an example value. Consider a manipulation of this variable, such that it now takes the value '4 – 8'. The cicada explanation seems to do a good job at allowing us to answer this counterfactual '*what if things had been different?*' question. If the ecological constraints were different, then the cicadas would have evolved to have life cycles of a different length: that is, the explanandum variable would take a different value.

[100] Information about the number of strawberries had by the parent (crucially, along with the mathematical background) tells us *exactly* what value the explanandum variable has. If the number is divisible by 3, then the explanandum variable will take the value 'equally distributable' (or 'possible', depending on exactly how we state the explanandum)), if the number is not divisible by 3 then the explanandum variable will take the value 'not equally distributable' (or 'impossible'). The value taken by the explanandum variable does, to be fair, tell us *something* about the explanan variable but not with as much specificity: if the explanandum variable takes the value 'possible' (or 'equally distributable') then we know that the value of the explanan variable 'number of strawberries had by the parent' takes as its value a number evenly divisible by 3, but we do not know what value it takes. In this sense, then, the counterfactual dependence remains asymmetric.

Moreover, the explanation can tell us exactly what those life-cycle lengths would have been. If there are any prime numbers that fall inside the range forced upon the cicada by the ecological constraints, then this would be the length of the cicada's life-cycle. So in the example given, the cicada would have evolved to have life-cycle length of 7 years.

Here's another example. Although this is somewhat supressed in the formalisation of the argument that Baker provides, recall that a crucial part of the explanation provided by biologists requires the suggestion that in the evolutionary history of the cicadas, there were either periodic predators or rivals. It is this fact that makes salient the number-theoretic theorem. Accordingly, the explanation also affords us knowledge of another dependence relation: between the fact that there existed periodic predators/rivals and the fact that a cicada species evolved to have prime periods. There are two counterfactual scenarios to consider. The first: what if there had been no other periodic rival species and no periodic predators in the evolutionary history of the cicada? The second: what if the periodic rivals and periodic predators had life-cycle lengths that were not *nearby* life-cycle lengths? The answer to both of these *w-questions* seems to be the same: the life-cycle lengths of the cicada species would not have been prime.[101] This is because, roughly, in such a scenario there would be no evolutionary advantage conferred upon the cicada by having prime life-cycles. On the modal account, explanatory power is grounded in facilitating the answering of what-if-things-had-been-different questions and it is the number-theoretic theorem that allows the answering of these questions. In this sense, the number-theoretic theorem helps to explain the explanandum.

This, then, is the basic idea. Taking the mathematical fact to play the role of the invariant generalization in Woodward-style explanations allows for a very natural extension of Woodward's account, using the conceptual resources of that account to accommodate the extra-mathematical explanations. That the above suggested renders the mathematical fact explanatory, and tells us *why* it is explanatory, is only a minimal requirement though. The details of the suggestion can be brought out by attending to three naturally occurring questions. First: does extending an account of explanation that appeals to counterfactuals to the extra-mathematical case mean that some of the counterfactuals are counter*possibles,* and if so, is this a problem? Second: how does the mathematical fact play the role of providing

[101] Or, more accurately, the life-cycle of the cicadas would *probably* not be prime – even had there been no rival species or predators, given the ecological constraints there is still a reasonable chance that the life-cycle length would end up taking the prime-numbered variable that falls inside the range.

modal information about the system being explained, if the mathematical fact does not seem to be *about*, or seem to *describe*, the target system in any obvious sense? Third: does extending an account of explanation usually applied to causal cases mean that the extra-mathematical cases are in fact causal, contra the near-unanimous assumption made in the literature? In the rest of this chapter, I attend to these questions.

4.4. The modal account and the role of counterpossibles

In this section I consider whether or not counterpossibles play a role in a modal account of extra-mathematical explanation and, against a trend emerging in forthcoming work by other commentators, argue that they do not.

There has been a recent flurry of work, developed in parallel, on the relationship between causal explanation, non-causal explanation, counterfactual information, and extra-mathematical explanation (Baron, Colyvan & Ripley 2017; Chirimuuta forthcoming; Jansson & Saatsi forthcoming). In addition to flagging awareness of this work, this section clarifies an outstanding point about the role of counterpossibles. In this section I vindicate an assumption made by Jansson and Saatsi (Jansson and Saatsi forthcoming) and argue against an assumption made by both Baron, Colyvan and Ripley (Baron, Colyvan & Ripley 2017) and Chirimuuta (Chirimuuta forthcoming): that is, I argue that a counterfactual, or modal, account of extra-mathematical explanation need not appeal to counterpossibles resulting from perturbations of the mathematical facts.

First, I offer a potential objection along the lines of the account having to deal in counterpossibles and then set out the ways in which Jansson and Saatsi, on the one hand, and Baron, Colyvan and Ripley and Chirimuuta, on the other, seem to take different positions on this issue. I then argue in favour of the former strategy. I argue that the counterpossible reading of the modal account mistakes the role played by the mathematical fact in the explanation. Clarifying this point, then, also constitutes a reply to this potential objection.

4.4.1. The counterpossibles objection(s)

Here, first, is a set-up of the counterpossibles question in terms of an objection. Counterpossibles are counterfactuals (or subjunctive conditionals) that take an impossible statement as their antecedent. Here's a (hopefully) uncontroversial example: if a given triangle T had three sides, then T would be a shape with the same number of sides as every square. This example demonstrates the sense in which (at least some) counterpossibles seem to pull us in two conflicting directions. There is, I take it, general assent that the antecedent of this conditional is impossible: it is impossible for an object to be a triangle and yet have four sides. Yet, there is *some sense* in which this counterpossible seems correct, even though there is no possible world at which its antecedent obtains (and it is therefore false that there is some set of relevant possible worlds at which the antecedent and conclusion are both true). In contrast, the counterpossible 'if a given triangle T had four sides, then Owen Smith would be Prime Minister' seems incorrect.

It is standardly assumed that mathematical claims are necessarily true if true and necessarily false is false.[102] Accordingly, *if* the modal account of extra-mathematical explanation requires us to consider what would be the case were the mathematical facts other than they are *then* the account requires countenancing counterpossibles. This generates two possible objections. The first claims that counterpossibles are vacuously true and mathematical counterpossibles are therefore incapable of doing any explanatory work, in virtue of being uninformative. This version of the objection involves endorsing the line on counterpossibles given by Lewis (Lewis 1991) and recently defended by Williamson (Williamson 2007; Williamson forthcoming), according to which it is false that counterpossibles can take non-vacuous truth-values. A second objection leaves open the question as to the semantics of counterpossibles but argues that just having to give an account of the truth-values of mathematical counterpossibles *at all* is a theoretical cost of the modal account. On this line, the modal account is undesirable because it requires arguing for a kind of view about counterpossibles endorsed by commentators who claim about counterpossibles can be non-vacuously true (Beall & van Fraassen 2003; Brogaard & Solerno 2013; Bjerring 2014; Ripley 2012).[103] This version of the objection makes fewer theoretical commitments – given that it leaves open the question about the semantics of counterpossibles – and is therefore less powerful for it. This second version of the objection, then, is a sort of argument via theoretical virtues: it identifies a theoretical cost of the modal account, rather than arguing directly that the modal account must be false in virtue of mathematical counterpossibles being uninformative. I will argue that both versions of the counterpossibles objection can be

[102] See Wright & Hale 1992 and Field 1993 for discussion.
[103] The authors listed here do not all agree on how one ought to extend the Lewis-Stalnaker semantics to cover impossible worlds, just that it is possible to do so.

defused by making the case that the modal account does not involve counterpossibles. The stronger objection is defused because the uninformative and trivial nature of counterpossibles becomes irrelevant and the weaker objection is defused because there is no burden on the proponent of the modal account to offer an account of the semantics of counterpossibles.

4.4.2. Recent work and disagreement on counterpossibles

In this section I give examples of two stances taken on the above question in recent and forthcoming work: Baron, Colyvan and Ripley, and Chirimuuta, take it that counterpossibles must be involved. Jansson & Saatsi, in contrast, do not mention counterpossibles, discussing only counterfactual that take as their antecedent statements about the target physical systems. After providing and discussing these examples, in the next section I aim to vindicate the latter absence. The account offered in this chapter, then, contributes to this emerging line of research by adjudicating this outstanding disagreement.

In a recent paper, Baron, Colyvan and Ripley also suggest that extra-mathematical explanation can be modelled counterfactually.[104] Unlike the approach taken above, they do not see themselves as extending any *particular* account of explanation that appeals to counterfactuals: Woodward's interventionist account is mentioned only as a passing piece of evidence for the claim that counterfactual information is taken to be closely related to, or to be identified with, explanatory information by many philosophers (Baron, Colyvan & Ripley 2017: 18). As I will argue in the next section, a failure to consider the details of accounts that put counterfactual information at the heart of explanation leads to the error of thinking that counterpossibles are involved in modelling extra-mathematical explanation counterfactually.

In the discussion, Baron, Colyvan and Ripley consider how we should think about perturbations (or what they call 'twiddles') of the mathematical facts: how much to keep stable, how modifications of facts about numbers ramify to change the nature of mathematical concepts like multiplication, and so on. The details of this sophisticated and

[104] Baron, Colyvan and Ripley do not give much attention to ramifications for the enhanced indispensability argument – they, too, see themselves engaged in the project of developing an account of extra-mathematical explanation. Given Colyvan's stake in the realist side of the debate, however, I take it that he holds that the account of extra-mathematical explanation he jointly offers bolsters the realist side even though he says nothing about why we should think that playing an explanatory role via offering counterfactual information is the kind of explanatory role that is ontologically committing. Perhaps this is an issue he will take up in future work.

interesting account do not bear, however, on the central question of whether or not extending a counterfactual account of explanation to the extra-mathematical case requires counterpossibles at all.[105] Baron et al. are explicit that they are not interested in offering an account of the semantics of counterfactuals (*ibid*: 2) and also that their contribution does not constitute a defence of "these familiar tools [counterfactuals] as an account of explanation at all" (*ibid*: 18) (directing the reader to Woodward 2003 (as well as Woodward & Hitchcock 2003 and Pearl 2000)) nor a defence of what they call "a counterfactual theory of extra-mathematical explanation", the proponents of which they note may include their "future time-slices" (*ibid*: 12). However, by taking as their starting assumption merely that counterfactuals are involved in explanation, rather than attempting to develop *some particular* account of explanation that appeals to counterfactuals (as I do above), Baron et al. go astray.[106]

It is worth noting, though, that Baron et al. are not alone in the forthcoming literature in making this assumption. Although her narrower focus is demonstrating that so-called efficient coding explanations from cognitive neuroscience constitute examples of extra-mathematical explanation, Mazviita Chirimuuta also takes it as read that extending a Woodwardian approach to the extra-mathematical case will involve counterpossibles. She says that:

> Instead, [in mathematical explanations] we are told how things would be under certain *impossible* scenarios in which the laws of mathematics are altered. This of course assumes that *counterpossible* statements – counterfactuals or subjunctive conditionals with impossible antecedents – can be non-vacuously true. (Chirimuuta forthcoming: 8)

Chirimuuta's discussion contrasts with Baron, Colyvan and Ripley's in that she discusses a particular counterfactual account of explanation (she makes explicit reference to Woodward's account): it coheres, though, in that it also maintains that extending such an account involves counterpossibles.[107]

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105 Even if the technical work done in Baron, Colyvan and Ripley 2017 is not required in order to give a modal account of extra-mathematical explanation, the work is not done in vain: how to model counterpossibles with mathematical or logical antecedents is an interesting philosophical question in its own right.

106 Baron makes much the same assumption elsewhere, stating that how plausible a counterfactual theory of extra-mathematical explanation is "depends a bit on how one feels about counterpossibles" (Baron 2016b: 468), before offering an objection to an account that appeals to counterpossibles.

107 Chirimuuta explicitly claims that the relevant counterfactuals are counterpossibles but it is not clear that this claim is borne out by her examples. She says that: "in other words, the information theoretic explanation of the efficiency of the brain informs us about counterfactual (or counterpossible) scenarios in which the laws of

In a forthcoming discussion, Jansson and Saatsi make explicit reference to Woodward's account, as Chirimuuta does, but demur in that they do not make any reference to counterpossibles.[108] Their discussion focuses on degrees of abstraction and targets the view that in the explanations we are interested in, information about *independence* is explanatory, rather than information about dependence (which is taken, in different forms, to be a view endorsed by Lange and Pincock). The debate, as set out by Jansson and Saatsi, is between exceptionalist accounts (to use the terminology of this thesis) according to which the relevant explanations work is done by facts about independence and anti-exceptionalist accounts that see the explanatory work being done by facts about dependence, which is more familiar. As discussed in §3.5 and fn.113, I have some qualms with this reading of the debate (and with their understanding of Pincock's view in particular), but in as much as they claim that these explanations should be understood as explanatory in virtue of providing counterfactual information (a la Woodward), I am in agreement. Nevertheless, in their discussion, Jansson & Saatsi do not explicitly state that they hold that extending a counterfactual account of explanation to the extra-mathematical case (or, as they term it, cases of abstract explanation) does not involve countenancing counterpossibles. However, given that the need to do so naturally suggests itself as an objection to such a view, I take their lack of discussion of this worry to signal that they hold that it is not required.[109] Whether or not the absence of a discussion of counterpossibles signals endorsement of the view that this is not a requirement,

information theory are different)", continuing that "the Shannon-Weaver definitions do not have the obvious modal strength of "13 is a prime number"" (Chirimuura forthcoming: 12). Two brief remarks should be made about this. First, it is unclear whether the relevant counterfactuals genuinely are counterpossibles: the antecedents appear to describe situations where the laws of information theory are different and, *prima facie*, worlds at which the laws of information theory are different are not impossible worlds. (A full discussion of the literature on the necessity of the laws of nature and the nature, in particular, of the laws of information theory would be required in order to claim this decisively). Some of the counterfactuals she considers seem transparently to not be counterpossibles: for example: "that the ability of the system to integrate sensory information for the duration of a trial is counterfactually dependent on it having a line attractor rather than a point attractor" (*ibid*: 21). Second: *even if* some of the relevant counterfactuals are indeed counterpossibles, the counterpossibles do not result from changes of *mathematical* facts, but rather changes in (something like) necessary conceptual truths about information theory. So, even though Chirimuuta explicitly agrees with Baron et al that counterpossibles are involved in extending a counterfactual account of explanation in this way, it is unclear from what she says whether or not she thinks that *mathematical* counterpossibles are at all involved in the interesting examples she sets out. (It will not do to say that because the Shannon-Weaver definitions may be stated mathematically, and that these definitions are the subject matter of the antecedents of the counterpossibles, the counterpossibles involve the mathematical facts being other than they are – as Chirimuuta notes, these are plausibly empirical facts stated mathematically rather than mathematical facts (*ibid*: 13)).

108 As Pincock does, Jansson and Saatsi refer to abstract explanations rather than extra-mathematical explanations: but, just as with Pincock's discussion, they cite some of the paradigm cases of extra-mathematical explanations and so it is safe to infer that they include these explanations (at least) as a *subset* of the explanations they are interested in.

109 The same absence is present in Saatsi's earlier discussion of this possibility (Saatsi 2016b: 1060-1063).

or whether it instead is simply something to be addressed in future work, in the next section I argue in favour of the former stance. By appending the view with such an argument, I adjudicate this disagreement (albeit, currently an implicit rather than explicit disagreement!) between emerging similar views.

4.4.3. Against the counterpossibility understanding

Those developing counterfactual accounts of explanation along the lines of this chapter assume *either* that counterpossibles are centrally involved in such an account or, it seems, that they are not involved at all. The fact that there are theoretical costs to taking one of these routes may well be playing a role. I think, however, this emerging disagreement is partially grounded by a fundamental disagreement between two different ways of accommodating the extra-mathematical cases into (something like) a counterfactual account of explanation. I have suggested here that the mathematical fact that features in a given extra-mathematical explanation should be understood as analogous to the invariant generalization, or law, present in the paradigm causal cases. This is not the only candidate for the analogy, however. A different way of accommodating the extra-mathematical cases is to think of the mathematical fact (and the corresponding mathematical objects) as analogous to the *causes* in the paradigm causal cases. This, recall, is implicit in Baker's dismissal of the possibility of causal accounts being any use, when he says "[the causal account] is incompatible with the existence of *any* genuine mathematical explanations, since mathematical objects (if they exist) are acausal (Baker 2005: 454).

With this distinction in mind, consider the following. There are two ways in which one might think that a modal account of extra-mathematical explanation would involve counterpossibles that result from holding fixed non-mathematical facts whilst changing mathematical facts. The first involves changes of the particular mathematical fact doing explanatory work: for example, considering how the explanandum variable would be different if prime periods *maximised* intersection. But now that the two ways of extending a Woodward-style account have been explicitly stated, this reveals itself to mistake the role played by the mathematical fact. I have suggested here that the fact should be understood as analogous to facts like the pendulum law: to invariant generalizations. In *Pendulum*, none of the counterfactual situations involve presupposing that the period of the pendulum and the gravitational field strength (and so on) stand in *different* relations to each other, rather than the relation expressed by the law. Rather, the pendulum law is held fixed and the variables are manipulated in order to understand the ramifications for the explanandum. As emphasised in the exposition of Woodward's account, *qua* account of causal explanation, it is the invariance of the generalization that is (partially) constitutive of its ability to aid in an explanation. This would be in tension with the suggestion that the mathematical fact in an extra-mathematical explanation does explanatory work by giving a user information about how the explanandum would take a different value if *this* mathematical fact were otherwise. Instead, I have suggested that it does work by giving us access to counterfactual information about the explans variables and how the explanandum variable depends on the values that they take.

Addressing with the second way that counterpossibles might enter into the picture is somewhat more fraught. This second way in which one might think that a modal account of extra-mathematical explanation would involve counterpossibles is if some of the manipulations involved variations of *particular* mathematical facts concerning, for example, primeness: for example, considering how the explanandum variable would differ if 7 were composite rather than prime.[110] In short, this challenge amounts to asking *why* we should endorse the law-analogy version of the modal account and not the cause-analogy version. So, here are some reasons. The first, *prima facie*, reason for not countenancing such counterpossibles involves fidelity to the presentations of the case studies in the scientific literature. In the presentations, biologists offer as the explanatory facts the facts about environmental factors, the existence of predators and the relevance of the number-theoretic result: in contrast, it seems as though the mathematical facts are held fixed. A second reason involves the form of the mathematical facts: they, just like the invariant generalizations, are general claims with very wide scope.[111] This has already been noted by Baker in his schematic extension of the D-N account of explanation (see §3.2).

It is important, third, to keep in mind that the counterpossibility reading of the modal account strictly involves *more* counterfactuals being invoked in understanding these cases, rather than an *alternate* collection of counterfactuals. A proponent of the counterpossibilist understanding of the modal account must, I take it, *also* think that counterfactuals involving

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[110] This is the kind of mathematical manipulation that Baron et al. consider to be involved. Baron makes much the same assumption in an earlier discussion (Baron 2016a) but again does not motivate endorsing the law-analogy rather than the object-analogy.

[111] Indeed, mathematical facts (like the number-theoretic and graph-theoretic theorems involved in *Cicadas* and *Bridges* respectively) can be seen as special cases of invariant generalizations.

the non-mathematical explanandum variables taking different values also enter into the explanation: this is required in order to render these facts explanatory and Baron, Colyvan & Ripley (although they say very little on this matter) mention "the non-mathematical twiddles" involved in their account (Baron, Colyvan & Ripley 2017: 15). Adding this additional class of counterfactuals (that is, the counterpossibles) requires motivation.

A response that that Baron, Colyvan & Ripley might give is the following. Counterpossibles concerning imagining what would happen if 13 had the property of being composite rather than prime (for example) are required because the mathematical object 13 is doing explanatory work and therefore the account must tell us how the explanandum depends on this object, 13. However, this perhaps subtly begs some important questions: once it is agreed that the mathematical fact does explanatory work by providing modal information, it seems an interesting open question whether or not it does so because 13 does explanatory work (and, therefore, its presence in the explanation means we ought to have the associated ontological commitment) or whether or not the mathematical fact is, whilst playing an indispensable role in the explanation, is nevertheless being used to express modal information *about the target system*, with this information being explanatory. In §4.6 I develop the account in a way that makes the second route look plausible.

The reason that adding counterpossibles to the set of relevant counterfactuals requires motivation is because doing so comes with an additional theoretical cost. A modal account of extra-mathematical explanation that involves counterpossibles has the cost of having to make sense of the notion of dependence relations holding between the subject matter of mathematics and the target system. In the counterfactual treatment of the paradigm causal cases, the counterfactual dependencies that permit the answering of *w-questions* are underpinned by patterns of causal dependence that exist in the world: they make true the invariant generalization which, in turn, allows the answering of the *w-questions*. As will be discussed in §4.8, I don't intend to argue here that the kind of causal dependence relations described by Woodward are the only kinds of worldly dependence relations: so too can relations like constitution and supervenience be used to make sense of variables counterfactually depending on each other.[112] But, the relationship between a mathematical

[112] The notion of grounding (Correia & Schnieder 2012; Fine 2012) may be helpful in spelling out the exact relationship between the counterfactual dependencies and the worldly dependence relations that are responsible for them, but this is not the only option.

object and the target system does not seem like it is characterised by any of these kinds of dependence relations.

There is a sense in which this debate cannot fully take place until commentators like Baron, Colyvan, Ripley and Chirimuuta make explicit why they think counterpossibles are involved rather than more familiar counterfactuals about the physical system. Until this is done, there is the suspicion that this emerges from the mistaken assumption that, in a causal explanation to the extra-mathematical case, the mathematical object should be treated as playing the role of the cause. In this section I have, hopefully, demonstrated that the burden is on proponents of a counterpossibilist understanding to offer these reasons and have, also, offered some *prima facie* reasons for thinking that they are mistaken in making this assumption.

4.5. Pincock's objection

So, on the account here, extra-mathematical explanation ought to be modelled along Woodwardian lines: the primary difference being that the fact doing the explaining is not an invariant generalizing giving access to counterfactual information in virtue of accurately describing causal dependence relations, but is rather a mathematical fact which, also, gives access to counterfactual information. Pincock describes his account as one that involves "two kinds of dependence relations" (Pincock 2015: 876) – his second kind of dependence relation is that of abstract dependence, discussed in the previous chapter, which involves the particular target system depending on some abstract object that it is an instantiation of:

> Woodward uses his account of manipulations and interventions to make precise what these [causal] dependence relations come to, my proposal is that an abstract explanation requires something similar. In addition to causal dependence relations, there are what we could call abstract dependence relations. (Pincock 2012: 877)

Whilst there is uncertainty about the notion of abstract dependence (as discussed in §3.5), this remark sounds consistent with much of what has been said in this chapter. Much as Woodward's account tells us how causal explanations provide counterfactual information by latching onto causal dependencies in the world, so too can the account tell us how extra-mathematical explanations provide counterfactual information by latching onto worldly dependencies.[113]

Despite this, Pincock takes Woodward's account of explanation to be insufficient for shedding light on the kind of explanation Pincock considers. Given the closeness of what Pincock says, in places, to a Woodward-style treatment, it is worth briefly considering the reasons he gives.[114] The first such reason is that discussed above: that because "nobody thinks that causal relations obtain in pure mathematics we have a non-causal explanation" (Pincock 2015: 867). As discussed above, this seems to import an unjustified account of the role of mathematical objects in extra-mathematical explanation. Furthermore, even if this point is accepted, the recent work on extending Woodward's account to different kinds of non-causal explanation suggests that this needn't be a barrier.

4.5.1. Pincock's objection

Perhaps for this reason, Pincock offers a second consideration against extending Woodward's account.[115] Pincock claims that, in the kinds of explanations susceptible to Woodwardian analysis, "we hold fixed the object mentioned in the explanandum and must be told how it would be different in appropriate counterfactual scenarios" (*ibid*: 868). Pincock argues that, in contrast, in the cases under consideration the value "lies instead in the *other-object* information it provides" (*ibid*: 868) – that the explanations do not "tell us how any

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113 Jansson and Saatsi say that Pincock "argues that none of the existing accounts with their focus on dependence can be extended to capture highly abstract explanations" (Jansson & Saatsi forthcoming: 2). It is, of course, correct that Pincock holds that existing accounts cannot accommodate (what he calls) abstract explanation. But it's important to stress again that this *isn't* because dependence is playing a crucial role in such cases. After all, Pincock's account gives a *central* role to dependence. What makes Pincock anti-exceptionalist, to my mind, is that he thinks that the kind of dependence at play in extra-mathematical explanation is not physical dependence and that this abstract form of dependence cannot be captured using Woodward's account. Whilst Pincock thinks that dependence plays a key role in both extra-mathematical and causal explanation (reading into his discussion an implicit endorsement that a Woodwardian approach is along the right lines when it comes to causal explanation), he thinks that the extra-mathematical cases require a bifurcation of the dependence relations at play in the two kinds of explanation.

114 One might reasonably wonder that, given that Pincock entertains the thought, at the end of his discussion, that "if we are willing to countenance at least two kinds of dependence relations, then abstract and causal explanations turn out to have more in common than initial appearances suggest" (Pincock 2015: 858), why is Pincock's view not anti-exceptionalist? It is, first, important to note that Pincock also says that "we can have as many kinds of explanation as there are dependence relations" (*ibid*: 876), suggesting that he does take abstract explanation to be a *sui generis* form of explanation. As one might expect, the answer to the original question turns on how, exactly, one draws the exceptionalist/anti-exceptionalist divide: on one carving, Pincock's suggestion is anti-exceptionalist because dependence relations (of various kinds) are involved in both causal and (what he calls) abstract explanations, but on another, it is anti-exceptionalist because he is explicit that existing *accounts* of explanation can't accommodate the case.

115 Pincock also discusses a consideration against extending the Kariatric account (Pincock 2015: 869-871).

particular physical system will change under an intervention" (*ibid*). I admit to finding this objection difficult to understand. Even if Woodward intends his account of explanation to yield explanations only of particular explanandum variables, it is easy to understand explanations of the behaviour of objects and systems *of a particular type* behaving in a certain way along the same lines (*cf.* Jansson & Saatsi forthcoming: 14-15).[116]

It is worthwhile considering an earlier objection Pincock offers along these lines, to try and get a grip on what the objection in Pincock 2015 might amount to. (It's not clear to me whether this objection, and the same-object objection in Pincock 2015 are the *same* objection approached from different sides – Pincock is unclear here and does not explicitly refer back to the discussion in Pincock 2012 - but given that it was somewhat difficult to get clear on the same-object objection, it is worthwhile discussing this version).

Pincock discusses *Bridges* in Pincock 2012 when discussing (what he calls) acausal representations – but his comments here might shed light on this objection to the *Bridges* case *qua* explanation. Pincock claims that *Bridges* is "clearly" not a causal representation, and then works through some of the standard accounts of causation. He says that "an advocate of a broad manipulation account might read some claims of counterfactual dependence into our representation and so insist that it is causal after all" (Pincock 2012: 53). Now, I am parking for the moment the question about whether explanations like *Bridges* are causal (see §4.8) – but other than that, a "broad manipulation account" seems to describe the account discussed in this chapter quite well. Pincock even makes remarks that resemble the kind of *w*-questions discussed above ("for example, some of the bridges could be a cause of the impossibility of the circuit because removing those bridges and shifting to a system [with an even number of bridges] would make the circuit possible" (*ibid*)). Pincock maintains that this suggestion cannot be made to work because the explanation "does not have this additional fact [about the ramifications of a bridge being removed for the possibility of completing a circuit] as part of its content precisely because it does not involve any other graphs besides

116 It's worth briefly noting that this response can be further supported by appealing to some things that Woodward says elsewhere. In discussion about the distinction between data and phenomena, Woodwards says that phenomena are "relatively stable and general features of the world which are potential objects of explanation and prediction by general theory" (Woodward 1989: 393) and include "weak neutral currents, the decay of the proton, and chunking and recency effects in human memory" amongst the examples (Bogen & Woodward 1988: 306). I introduce these examples here not to take a stand about whether data or phenomena are the proper objects of explanation and prediction. Rather, they are presented as evidence that Woodward takes rather general features of the world to be the objects of scientific explanation and that this is perfectly consistent with what Pincock says about the case study he describes.

the one pictured [referring to a graph of a non-tourable bridge system]. Yet, on the face of it, this is just as puzzling as the same-object objection (or, the same-object version of the objection): if I explain why a window broke by demonstrating that its breaking depended on a rock being thrown at it, there is a weak sense in which this counterfactual is describing a different window (one that did not have a rock thrown at it), but there is *also* a perfectly naturally sense in which I am providing information *about this very broken window*!

There *are* some counterfactuals that Pincock thinks count as "part of" the explanation, but he says that they are "quite special and should not be mixed in with the sorts of counterfactuals that are central to causal representations" (*ibid*: 54): an example is the fact that the physical constitution of the bridge is not something that its tourability counterfactually depends on. Pincock says that it is not coherent to ask how the bridges under examination would change if they had a different physical constitution because "it is arguably essential to the bridges that they be made of stone. So there is no possible manipulation of these bridges that would change their constitution" (*ibid*: 54). Perhaps this drives a wedge between the *Bridges* case and the broken window case: but if Pincock's objection *is* grounded by a substantive view about essential properties, he should do more work to fully spell out what this view is and how it is motivated. Until this is done, the proponent of a Woodward-style account can simply say that information about what the explanandum does *not* depend on is *still* information about which parts of the system the explanandum depends on and so, putting aside essentialist views about physical constitution, these counterfactuals are not, contra Pincock, "special".

4.6. The modal account and the *in virtue of* question

In this section I set out a remaining question about the claimed analogy between the role played by the invariant generalization in paradigm causal explanations and the role played by the mathematical fact in extra-mathematical explanations. Drawing on some recent remarks by Mary Leng, I also suggest a possible answer to this question.

4.6.1. The question

Let's take stock. So far, I argued that *prima facie* reasons for not extending causal accounts of explanation to the extra-mathematical case are unpersuasive. I then endorsed a general picture on which that Woodward's account of explanation can be extended to accommodate the extra-mathematical cases, and that doing so does not require appealing to counterpossibles resulting from pertubations of the mathematical facts (in line with some recent suggestions on these lines and against others). There are a couple of remaining questions that must be addressed.

Suppose one asks the following question of the proponent of the counterfactual account of causal explanation: *in virtue of what* does the invariant generalization that features in causal explanations allow a user access to counterfactual information and to the answers to *w-questions*? Call this the *in virtue of* question. A natural answer suggests itself: the generalization *just is* a fact about the world – in particular, it is a fact about dependence relations that hold in the world, which underpin the counterfactual dependence relations that a user of the explanation is given access to. It is by latching onto this information that that part of the explanation does explanatory work. The generalization allows the answering of *w-questions* because it accurately describes (part of) the world's system of dependence relations (and, in the canonical cases, the world's system of *causal* dependence relations). In *Pendulum,* the pendulum law accurately describes the dependence relations between the length of the pendulum, the gravitational field strength and so on. If the pendulum law expressed a false claim about the world's causal dependence relations, the thought goes, we would have no explanation. There is a very tight connection between the truth of the generalization featuring in a paradigm causal explanation and the counterfactual dependence relations.[117]

So: a natural answer suggests itself to the *in virtue of* question in the regular causal case. However, the same answer, it seems, cannot be given in the extra-mathematical case. On their surface, at least, the mathematical facts involved in extra-mathematical explanations do not describe worldly patterns of dependence: (at least some of) the facts in these cases are purely mathematical in character and describe the behaviour and properties of the subject matter of mathematics.[118] The graph-theoretic fact, if it exhibits information about dependence at all, tells us how properties of graphs depend on other facts about graphs: the graph-theoretic fact is *about graphs*, in the same way that the pendulum law is *about* pendulums (of a certain kind). What, then, is the connection between the mathematical fact and the

[117] Indeed, for the Humean about laws, the fact about dependence expressed by the law-like statement is made true just by the relevant set of facts.
[118] This is complicated by the fact that, for example, the number theoretic theorem in *Cicadas* appeals to the notions of 'periods', a biological phenomenon.

target system? Whatever grounds the truth, or correctness, of the mathematical fact, it is decisively *not* facts about worldly dependence.

4.6.2. Towards an answer to the question

What is required is an account of *in virtue of what* a mathematical fact (jointly with other non-mathematical facts) exhibits information about counterfactual dependence, in the same way that an invariant generalization does by directly describing the world's patterns of dependence. In this section I tentatively offer an answer to this question, drawing on some recent remarks by Mary Leng. The answer raises some additional questions of their own but, felicitously, at least some of these questions will naturally be addressed in chapters 5 and 6.

In recent work, Leng has stressed the structural nature of mathematical explanations.[119] This seems to stop short of being an *account* of mathematical explanation – little is said about whether or not mathematical explanations being structural in this way means that they are *sui generis* (that is, whether Leng's remarks *qua* account of mathematical explanation mean that the resulting account is anti-exceptionalist) or whether other, non-mathematical, explanations are taken to be structural in the same way. For this reason, these remarks can be understood as describing a characteristic of extra-mathematical explanations, one that can be drawn on to flesh out aspects of different accounts.[120] In using this feature of extra-mathematical explanations to answer the *in virtue of* question, I therefore incorporate this Leng's insight into an existing account of explanation.

The general idea behind Leng's structural suggestion is that the target system involved in a given extra-mathematical explanation instantiates a mathematical structure (described by specifying the axioms that govern that structure). This is what connects the mathematical fact to the non-mathematical target system and makes the mathematical fact capable of providing modal information about the explanandum even though the fact does not describe the dependency relations found in the world, like an invariant generalization does. Leng says the following about structural explanations:

[119] Although Pincock does not explicitly build this notion into his descriptive account of extra-mathematical explanation discussed in the previous chapter, he does appeal to this fact in his discussion of limits of inference to the best explanation (Pincock 2012: 211-217).
[120] Indeed, the fact that extra-mathematical explanation has this structural feature seems strongly related to Pincock's sense that the explanations are distinct in virtue of being in some sense abstract.

> We can think of a mathematical structure as characterized by axioms. A physical system instantiating that structure is one where those axioms are true when interpreted as about that physical system. A structural explanation will explain a phenomenon by showing that (a) that the phenomenon occurs in a physical system instantiating a general mathematical structure, and (b) the existence of that phenomenon is a consequence of the structure-characterizing axioms once suitably interpreted. (Leng 2012: 990-991)

There is a sense, then, in which this response to the *in virtue of* question involves disputing the central motivating observation: that whilst the invariant generalization does its work by describing the world's patterns of dependence, the mathematical fact must be doing something very different. On this line, the mathematical fact *is* characterizing or describing the world, in virtue of this part of the world being an (approximate) instantiation of the relevant axioms that *also* characterize the mathematical domain in question. This allows the dependence relations in the mathematical domain *carry over* to the non-mathematical domain: a dependence relation between two numbers and intersection will also be a dependence relation between the intersection and two life-cycles that have these lengths. Grasping the mathematical fact, and grasping that the target system being explained is an (approximate) instantiation of the axioms that characterise the domain that the mathematical fact describes, gives us access to a set of counterfactuals about the target physical system. And, as I suggested above, it is in virtue of having access to these counterfactuals that we have an explanation at all. Now, not all of what I have said here is consistent with the letter of Leng's schematic presentation of this structural account (*ibid*: 988-991) At places the account is described as akin, in some ways, to the DN account of explanation (*ibid*: 988). As noted, Leng does not say whether or not she takes structural explanations to be distinct in kind from other kinds of explanation. I have suggested here that her insights about the structural nature of these explanations need not lead one into thinking that such explanations are *sui generis*: rather, they can be appealed to in order to explain *why it is that* a mathematical fact provides a user with information about counterfactual dependence. The result is an account of extra-mathematical that appropriately draws on the structural character of such explanations but nevertheless locates the explanatory power of the explanation in the modal information it conveys. No doubt there are existing questions to be answered by this answer to the *in virtue of* question. Some of these (such as what it means to say that the target system has a structure such that it is true to say that the structure of the target system (approximately) instantiates a collection of axioms) will naturally rear their heads again in chapter 6, whilst others will have to wait for future work. This will be sign-posted in more detail in the thesis conclusion.[121]

In this section, I have raised an outstanding question for any attempt to extend the modal account of explanation to the extra-mathematical case and made some steps towards providing an answer to this question. I will return to this question in the conclusion of the thesis, when I discuss the ramifications of this view about extra-mathematical explanation for the debate about mathematical ontology, and for the relationship between mathematics' representational and explanatory capacities.

4.7. The modal account vindicated

The criteria set out at the end of chapter 2 were used, in chapter 3, to argue that rival accounts are unsatisfactory. In this section I argue that the modal account fares better in comparison.

As the worked examples from §4.3. demonstrate, it seems as though the modal account tells us in virtue of what the mathematical fact in the explanation is explanatory: the mathematical fact is explanatory in virtue of providing counterfactual information about the explanandum, with which users of the explantion can answer *w-questions*. The modal account, then, can accommodate the case studies. Recall, also, the requirement introduced in this chapter after the discussion of the causal information present in many extra-mathematical explanations: that a successful account of extra-mathematical explanation must demonstrate how *both* the causal and mathematical explanans are explanatory. As well as the fact about the non-divisibility of 23 by 3, the facts about the number of strawberries had and the number of children also play a role in the explanation and our account of extra-mathematical explanation ought to respect this. It seems that the modal account can do so, and can do so in a unified way. In *Cicadas*, for example, both the causal information about the ecosystem and the existence of periodical predators and the mathematical fact explain by belonging to a collection of facts that exhibit information about counterfactual dependence and permit

[121] In a recent discussion, Baron considers and then rejects an account of extra-mathematical explanation that he calls the 'mapping' account (Baron 2016b: 464) – it is worthwhile distinguishing what is said here from the sketch he provides. The kind of account Baron considers takes a piece of mathematics to explain some fact just in case there is a structural mapping between the mathematics and the target system: Baron rejects it for the obvious reason that it is trivial to generate cases of structural mappings and that "such a case would, on the mapping theory, count as an extra-mathematical explanation just because it features a morphism" (*ibid*). It should be clear that on the account offered here, the holding of a structural mapping between vehicle and target is not sufficient for the mathematics to be explanatory.

the answering of *w-questions*. It is not the case, then, that the account falls into the trap of yielding the outcome that *only* the mathematical information in the explanation is explanatory.

What, then, of the second requirement: that the successful account of extra-mathematical explanation shed light on the different kinds of mathematical explanation that there are? Recall that on the entailment account (see §3.2) both *Pendulum* and *Cicadas* came out as being cases of a mathematical fact explaining a non-mathematical fact, in virtue of their both being mathematical sentences that (together with non-mathematical facts) jointly entail the explanandum. One might worry that this is also a problem with the modal account. If both the simple pendulum law and the number-theoretic theorem do their explanatory work by allowing a user of the explanation to answer *w*-questions, what is the difference between the explanations that appeal to these two facts? If it is a desideratum that an account of extra-mathematical explanation explains why these explanations are judged to be importantly different, it seems as though the modal account fails.

This worry can be responded to by drawing attention to the difference between invariant generalizations and mathematical facts that was drawn out and commented on in the previous section. The simple pendulum law is mathematically stated but, as discussed above, seems to have as its content a claim about a physical regularity, concerning the relationship between period length and gravitational field strength and so on. Until its terms are interpreted, it seems that the simple pendulum law fails to express any content: it is only true and false *of* the physical world rather than expressing some mathematical fact. The fact being expressed (and expressed mathematically) is the invariant generalization – the description of the world's patterns of counterfactual dependence. This is not the case of the kinds of mathematical facts appealed to in the extra-mathematical explanations. The graph-theoretic fact appealed to in *Bridges* and the fact about primes appealed to in *Cicadas* are mathematical facts rather than non-mathematical facts expressed using mathematics. This, then, is perhaps how the modal account can cleave apart mathematical explanations from mathematicised explanations. In mathematicised explanations, the explanatory work is being done by a physical regularity which is expressed using mathematics but in the cases of mathematical explanations, it is a *mathematical* fact that allows us access to modal information.[122]

[122] Other accounts of extra-mathematical explanation could, of course, also appeal to this difference between the mathematical facts appealed to by extra-mathematical explanations and the non-mathematical facts expressed mathematically by mathematicised explanations. Two brief comments: this distinction is naturally

4.8. The modal account of extra-mathematical explanation and causation

In the preceding sections I have made the case that Woodward's account of causal explanation can plausibly be extended to accommodate the extra-mathematical cases, adjudicated an emerging disagreement between recent accounts making suggestions on these lines. A mathematical fact contributes to the explanation in the same way that an invariant generalization does in the paradigm causal cases: by (jointly with the other explanans) exhibiting counterfactual information and allowing the user of the explanation to answer *w-questions*. I have also tentatively suggested an explanation as to *how* the mathematical fact can play this role, even though the mathematical fact does not describe the world's dependence relations in the same way that an invariant generalization does. A natural question to ask is: does this mean that extra-mathematical explanations are causal, after all? §4.1 cleared up some *prima facie* concerns about extending a causal account to these cases, but this does not, of course, amount to a positive case.[123]

There is an obvious sense in which one's answer to this question depends on what account of causation is endorsed, and this is not a thesis about causation. Two substantive points can be made, though. To claim that a causal theory of explanation can be extended to the extra-mathematical case is distinct from making the claim that mathematical objects *cause* the explanandum to take place. I expect that *this* is the view that philosophers balk at when considering extending a causal account in this way: both our intuitive notion of causation (in as much as we have one) and many accounts of causation will forbid this.

The fact that Woodward's account of causal explanation has been extended suggests that what we should be asking is whether or not the interventionist notion of causation means

suggested *by* the modal account and, importantly, none of the accounts in the previous section were rejected *only* because they could not answer the question about how to distinguish extra-mathematical from mathematicised explanations.

123 As is clear from the discussion in the previous chapter and in section two of this chapter, Pincock and Lange explicitly take these explanations to be non-causal. Jansson and Saatsi do not respond to this question, taking it to be orthogonal to their concerns (Jansson and Saatsi forthcoming: fn.1) Whilst the answer to the question is, of course, orthogonal to the task of understanding how extra-mathematical explanations function, it *does* bear on questions about how these relations relate to other kinds of explanation and how they are similar and dissimilar. As signalled in the introduction, this is one of the background questions that can hopefully have light shed on it by this thesis, so I briefly consider it here. It is to no detriment of Jansson and Saatsi's discussion that they do not.

that the extra-mathematical cases are causal explanations. The answer to this question turns on something that was noted earlier, in §4.2. Recall that, for Woodward, the relevant set of interventions contain all the *actually possible* manipulations of the explanans variables that we could carry out but also some others: the meaning of 'intervention' is extended in a way that stops short of including *all* the (logically) possible manipulations of explanans variables but contains *some* manipulations that we are not *in fact* able to carry out.

Assuming the success of the modal account of extra-mathematical explanation set out above, and assuming something like the interventionist account of causation, whether or not a given extra-mathematical explanation counts as causal (in this, plausibly quite narrow and technical, sense) depends on whether or not the manipulations of the explanans variables count as interventions. Chirimuuta also takes this question to turn on whether or not the changes in explanans variables can be understood as resulting from manipulations (Chirimuuta forthcoming: 6).

One way of considering whether or not a change of explanans variables ought to count as a intervention (and therefore whether the explanation ought to count as causal, assuming an interventionist understanding of causation) is to consider whether the change has some other hallmarks that we associate with causation. For example, Chirimuuta suggests that a particular coding explanation from cognitive neuroscience ought to count as non-causal because "there is no spatio-temporal separation between putative cause and putative effect. To change the dynamical properties is just to change the information processing capacities of the network" (Chirimuuta forthcoming: 21) and Pincock also ties the notions of cause and effect to temporal notions in his discussion of causal and acausal representations (Pincock 2012: 52-53). I will park, for now, issues about whether or not introducing these temporal aspects is faithful to Woodward's presentation of the notion of a manipulation and reflect on what ramifications there would be for carving up the case studies of extra-mathematical explanation into causal and non-causal explanations. The lack of spatio-temporal separation between the explanan manipulation and explanandum variable taking a different value seems to apply to *Bridges* but less well to the optimality examples. This would vindicate the claim above that whether or not a given extra-mathematical explanation counts as causal depends on the details of the dependency relations involved in the explanation.[124]

[124] Lange, of course, would argue that the lack of spatio-temporal separation is more evidence that the genuine extra-mathematical explanations are not the optimality explanations that have been taken to be canonical: see fn.87 for details.

In contrast to this conservative approach, recent work in the mechanist literature has suggested that some seemingly non-causal worldly dependence relations (ones that can underpin counterfactual dependence relations between explanan and explanandum values) standardly taken to be non-causal can, in fact, be taken to be causal on an interventionist reading. Here's one example of this kind of strategy, intended to be illustrative. It is standardly taken that in mechanistic models, mechanisms take part in causal relationships but the relationship between the mechanism and its components is one of constitution and, further, in virtue of being a relationship of constitution the relationship between a mechanism and its components cannot be a causal relationship. This, it is often held, is in virtue of the fact that the relations stood in by the behaviour of the mechanism (taken as a whole) and of the mechanism's component parts are synchronic, symmetric and involve non-distinct variables (Craver & Bechtel 2007; Craver 2007; *cf.* Harinen 2014). However, in a recent discussion, Harinen argues (introducing the notion of *causal inbetweennness*) that on a principled extension of the notion of an ideal intervention, these constitution relations between the mechanism and its components can be understood as causal, on an interventionist understanding of causation (Harinen 2014: 13-17). Here, I have elided many of the interesting details about this accommodation of seemingly non-causal constitutive dependence into a causal account: it is presented here merely as evidence that, as well as conservative claims that seemingly non-causal dependence relations are indeed non-causal, those in this literature are willing to consider extensions of the notion of an intervention that allow synchronic and symmetric dependence relations to come out as causal, after all.

For a given kind of (seemingly) non-causal dependence, then, it seems an open question as to whether or not there exists a principled extension of the notion of an ideal intervention such that, under this extension, the kind of dependence comes out as (non-paradigmatically) causal. I can't, here, offer an account of when we should or should not think that extensions of terms have determinate contents and how we can come to know this. In addition to these two options (one conservative, one liberal), there is a third option that it is worth briefly mentioning.

Much of Mark Wilson's *Wandering Significance* is concerned with what he refers to as 'tropospheric complacency', which Maddy helpfully unpacks as the fact that "we tend to think that our concepts […] mark fully determinate features or attributes, that there is a determinate fact of the matter as to where they apply and where they don't, that this is so even for questions we haven't yet been able to settle one way or the other" (Maddy 2011: 106). It's worth briefly considering one of Wilson's examples, that of ice:

> Water, in fact, represents a notoriously eccentric substance, capable of forming into a wide range of peculiar structures. (Wilson 2006: 55)
>
> The author [of a textbook] doesn't regard the clathrate structure as true ice […] but is it clear that our everyday conception of ***ice*** *requires* – as opposed to *accepts* – this distinction? (I, for one, had never thought about such matters at all.) (*ibid*: 55-56) (emphasis and bold in the original)

As Maddy notes in her discussion of Wilson, his point is that:

> nothing in our ordinary use or understanding of the term 'ice', indeed nothing in the underlying chemical facts that we subsequently discover about the many ways water can form into a solid – in short, nothing in our heads, in our language, or in the world will force either answer to this question" (Maddy 2011: 107).

Perhaps the case of extensions of Woodward's notion of an intervention is analogous to the open question about the extension of the use of the term 'ice' in Wilson's example. On this line, nothing "in our heads, in our language, or in the world" forces us to decide that the changes of explanans variables in the extra-mathematical cases either do or do not count as interventions. If interventions had to be those that we could, in practice, carry out then this would decide the matter – but as discussed, this is not the case. It may be the case that the changes in explanans variables in some cases (say, *Bridges*) are such that we are inclined to judge them to fall outside the extension of 'intervention' whilst those in *Cicadas* (such as intervening to change the environmental conditions that affect the life-cycle lengths) fall within the extension. But, again, it isn't clear that any particular answer is forced upon us either by our standard use of 'intervention' nor scientific practice.

I have briefly surveyed two possible ways of addressing the question of this section. The first is to suggest some criteria with which we might determine, in a given case, whether or not a particular change to an explanans variable counts as an intervention in the technical sense of Woodward's account of explanation. This might vindicate the assumption that the extra-mathematical cases are non-causal or it may, like with Harinen's treatment of constitutive explanation, demonstrate that extra-mathematical explanation is causal after all. The second kind of way of addressing this question is to suggest that there are no facts that dictate a particular answer to such a question. Whether or not a given case of extra-mathematical explanation counts as causal or non-causal, then, depends both on there being a determinate answer to what the proper modification of the notion of an ideal intervention is and, then, if the answer to this question is 'yes' whether or not this modified notion counts the changes of explanans variables as interventions. I will return to these questions in the conclusion of the thesis and indicate where work should be done in order to make progress towards an answer.

The fact that the account here does not decisively settle the question as to the causal status of extra-mathematical explanations (at least, not without being supplemented with a set of rules that govern how 'manipulation' can and cannot be extended to edge cases) does not threaten its status *qua* account of extra-mathematical explanation. As discussed briefly in the introduction, the existence, and nature, of non-causal explanation is an area of burgeoning research. Future work should draw both on the account here, and on the various demarcations and discussions in this literature, to adjudicate more decisively whether or not there is a substantive answer to the causal status of extra-mathematical explanations and, if the answer is affirmative, what this answer is. That the account here may leave it open as to whether or not any of these explanations are causal may be unsatisfying in some sense: but it is important to note that it leaves practice as it is and, nevertheless, sheds light on how these explanations function.

4.9. Conclusion

The mathematical fact in extra-mathematical explanations does explanatory work by allowing a user of the explanation to answer *what-if-things-had-been-different* questions. The explanatory practice found in these cases, then, is continuous with other forms of counterfactual explanation in science: where the exceptionalist thinks we have two fundamentally different kinds of explanation, we in fact find one. The future of shedding light on extra-mathematical explanation is surely by paying close attention to our existing accounts of explanation. I have argued here that by failing to pay attention to the particularities of such accounts, trading only on the notion that explanation is often related to counterfactual information, Baron et al. go wrong by thinking that the mathematics in an extra-mathematical explanation must play a role analogous to the cause in the paradigm causal cases. Yet, when we turn to Woodward's account, it is far more natural to understand the mathematical fact as playing the role of the invariant generalization. Having located what I take to be the source of this apparent disagreement between these recent suggestions in the literature, I argued that Jansson & Saatsi's contribution is the more plausible accommodation of these extra-mathematical cases into a Woodward-style account. More generally, the moral is that extending causal accounts of explanation to the extra-mathematical case need not take the mathematical object to be playing the role of the cause.

In this chapter, then, I have offered a positive proposal, offered a corrective to some recent proposals that plough the same furrows, and discussed what needs to be done in order to answer the question about the causal status of extra-mathematical explanations. Some open and pressing questions remain about the account: I have suggested an answer to the *in virtue of* question here and will return to some more questions in the thesis conclusion. Before moving on, a natural question is: what are the wider ramifications of this fact for the background debates in the philosophy of science and mathematics?

Consider, first, the debates about exceptionalism and anti-exceptionalism, and monism and pluralism. The success of the modal account of extra-mathematical explanation vindicates a form of anti-exceptionalism. Although there are differences between Woodward's account of causal explanation and the account of extra-mathematical explanation endorsed here, the same kind of information is explanatory. The account here is one part of a piecemeal defense of explanatory monism. Consider, second, the debates about ontology and explanation. On the one hand, the account here seems to vindicate the claim that mathematical facts play a crucial role in scientific explanation: extra-mathematical explanations did not end up being surrogates for underlying, more fundamental, non-mathematical explanations and the mathematical fact in extra-mathematical explanations and the invariant generalizations involved in (paradigm cases of) causal explanation play the same kind of explanatory role in substantively different ways. On the other hand, in answering the *in virtue of* question, it seemed that although the mathematics is (plausibly indispensably!) involved in the explanation, it plays its role by helping us grasp facts about the target system. I will return to this question after discussing, in more detail, mathematical representation. This is the task of the next two chapters.

Chapter 5

Scientific and epistemic representation

In the next two chapters I make steps towards shedding light on the representational capacity of mathematics. This is motivated by two aspects of the discussion so far, and it is worthwhile recalling them here. In chapter 1, I argued that in order for the Representation Conditional to be assessed, an account of mathematical scientific representation must be produced: without this, it is impossible to assess the representationalist nominalist inference from Representationalism to the claim that our world-oriented uses of mathematics do not commit us to realism. In short, an account is needed if we are to find out whether the representational capacity of mathematics is ontologically innocent. In chapter 4, I argued that the modal account of extra-mathematical explanation must be supplemented with an account of *in virtue of what* a piece of mathematics can convey modal information. I argued that one natural way of making sense how mathematical facts can play this role is to appeal to the idea that the natural numbers stand in structural relations to the life-cycles of cicadas measured in years, that the structure of the connected graph is instantiated by a concrete bridge system, and so on. In the next chapter, I consider attempts to make this hunch precise, by considering recent work on mathematical scientific representation that appeal, in one way or another, to structural similarity.

However, turning to the recent literature on scientific representation understood more generally, there are two challenges that appear to undermine both the narrow focus of accounts of mathematical representation and their reliance on structural similarity. Callender and Cohen argue that it is a mistake to think that there are any interesting questions about scientific representation that are not also questions about representation more generally. As is clear, their view about representation equally threatens attempts to say anything interesting about *mathematical* scientific representations in particular. Suárez and Frigg separately provide a set of objections that claim to undermine accounts of scientific representation that give a pivotal role to structural relations, or relations of similarity. Yet, these notions are crucial to recent accounts of mathematical representation, as will become apparent in chapter 6.

I consider the worries of Callender and Cohen, and Suárez and Frigg, in this chapter, prior to the discussion of particular accounts of mathematical scientific representation in the next chapter, for three reasons.

The first is that these worries are in some sense foundational, or at least *prior*, to the questions addressed by the accounts considered in the next chapter. The second reason is that, as I will argue below, accommodating the two objections places constraints on what accounts of mathematical scientific representation look like. In particular, they place constraints on what sorts of relations between vehicle and target are responsible for different facts about the representational vehicle. I will say much more about what these features of a representation amount to in this chapter and in the next. Accordingly, it should be concluded neither that Callender and Cohen, and Suárez and Frigg's objections entirely miss the mark nor that they genuinely undermine work on mathematical scientific representation: rather, that they are insights that can guide such an investigation. The third reason concerns the anti-exceptionalist hypothesis. Just as extra-mathematical explanation can be accommodated by (an extension of) an existing account of explanation, in the next chapter I argue that mathematical representation can be accommodated by an existing account of representation. Accordingly, it is appropriate to set out this account of representation and this is naturally done in the context of discussing scientific representation *simpliciter*.

In section one I set out some examples of representations. In section two I set out Callender and Cohen's reductionism about scientific representation. In section three I first discuss the reasons that Callender and Cohen give for accepting reductionism. I then argue that there are questions to be asked about scientific representation, contra Callender and Cohen that (a) are not questions about the representational capacity of mental states (b) are not questions about the relationship between the representational capacity of derivative representations and the representational capacity of mental states and (c) are plausibly not questions the answers to which are determined wholly by the pragmatics of representational practices. This is in virtue of scientific representation being a form of epistemic representation. Accordingly, in section four I discuss Contessa's interpretational account of epistemic representation and how it should be naturally understood as answering some special problems of scientific representation.

In section five I set out Suárez and Frigg's objections that purport to threaten any account of representation that appeals to notions of structural similarity is fatally mistaken. In section six I argue that Suárez and Frigg's objections fail because they mistake the role that structural relations ought to play in an account of epistemic representation. I then conclude.

5.1. Examples of representations

In this section I set out some examples of representations.[125]

IKEA instructions

Consider the following collection of pictures, found in a set of IKEA instructions.

Image removed due to copyright.

Figure 1. IKEA bookshelf instructions

The first distinctive feature is that, unlike lots of the other representations considered in this thesis, it is entirely non-linguistic and entirely non-mathematical.[126] The second is that it provides the user with information about the target: namely, how to arrange its parts such

[125] Note: representational vehicles are sometimes known as representational sources and representational vehicles are sometimes known as representational objects (Suárez 2003; Suárez 2004).

[126] The above image includes the IKEA logo, which is linguistic: but it seems reasonably safe to not include this as part of the representation.

that they result in a stable bookcase. Whilst other pictures often give a viewer information about its target, it is the *function* of the IKEA instructions to do so. If a portrait is accurate, then one can learn facts about what its target looks like and one can discern at least one fact about the referent of a linguistic object from considering only the object (namely, that it is referred to by that linguistic object, in some contexts) it is not the *function* of these representations to allow a user to learn about the representational target. The third distinctive feature is that it is clearly not a scientific representation.

The Glasgow subway map

Consider the following map of the Glasgow subway system, known to some (but not to any locals) as the Clockwork Orange.

Image removed due to copyright.

Figure 2. Glasgow subway map

The target is the Glasgow subway system: a complex physical object, or collection of physical objects, constituted by stations and rail tracks.[127] The representational vehicle is the collection of symbols and words that, spatially arranged in a certain way, jointly constitute the map.[128] The first distinctive feature of the representation is that it can be used to learn facts about the subway system. Whilst the subway map and the linguistic object 'the Glasgow subway system' both denote the Glasgow subway system, only the former can be used to infer facts

[127] Perhaps the relations between the stations are *part of* the Glasgow subway system or perhaps they are merely relations between parts of the representational target without being *part of* the target itself.
[128] I take it that the same map can have many instances but that not all maps of the same representational target are identical. I doubt that there is anything fully general that can be said about identity conditions for representational vehicles, other than that identity of representational targets is insufficient. Identity conditions of mathematical representations and for pictorial representations, for example, will differ greatly.

about the Glasgow subway system. The second distinctive feature is that we would not naturally refer to the subway map as a scientific representation. A third distinctive feature is that it is only partially linguistic. Whilst the linguistic objects ('Bridge Street', 'Inner Circle' and so on) carry some of the representational burden, so do its topographical properties: the linguistic objects without the symbols will fail to represent in the same way, so would the linguistic and symbolic objects arranged differently.

The Lotka-Volterra equations

The Lotka-Volterra equations constitute a mathematical representation of certain features of a biological system. In particular, the differential equations represent the effect of predation and reproduction on predator and prey populations:

$$\frac{dV}{dt} = rV - (aV)P$$

$$\frac{dP}{dT} = b(aV)P - mP$$

In the above equations, V is the number of predators, P is the number of prey, r is the rate of reproduction for the predators, m is the rate of death for the prey, a is a parameter representing how many prey are eaten by existing predators and b measures the rate at which consumed prey are converted into new predators. The equations capture that an increase in prey results in an increase in prey eaten by a predator which, in turn, results in an increase in the number of predators (via reproduction) and that there being *too many* predators will reduce the prey population which, in turn, reduces the population of predators.

The first interesting aspect of the equations is that the target of the representation can be different in different contexts – I say more about this in §6.7 when discussing the example in more detail. Second, the representation can be used to investigate the predator-prey populations: as Weisberg notes, the representations can be used to demonstrate the Volterra principle (Roughgarden 1979: 439): "the population of prey will increase relative to the number of predators upon application of a pesticide" (Weisberg 2006: 735). Third, it is unclear whether or not Lotka-Volterra equations are a causal representation. Pincock suggests that "no biologist should take it to be an accurate representation of the causes operating in any biological system (Pincock 2012: 59): however, the considerations offered in §4.5 seem to transfer over to this case of representation.

The Phillips hydraulic machine

In the late 1940s, Bill Phillips developed a hydraulic model of the macroeconomy. The representational vehicle is a physical 3D model ("the most famous 3D model in economics" (Morgan & Boumans 2004: 369)). Exactly what the representational target is unclear: it will differ in different contests.[129] As Morgan & Boumans explain, the model "represents the macroeconomy by flows and stocks of coloured water in a system or perspex tanks and channels" (Morgan & Boumans: *ibid*). The first distinctive feature of the hydraulic machine is that it is a scientific representation.[130] Second, the hydraulic machine itself is an entirely non-linguistic and non-mathematical representation: what does the representing are physical objects. Third, the hydraulic machine is constructed with the aim of being able to infer from facts about the machine to facts about a given economy (or to facts about all economies satisfying certain conditions).

5.2. Reductionism and the non-specialness of scientific representation

In this section I consider Callender and Cohen's argument concerning the specialness of scientific representation.

5.2.1. Callender and Cohen's reductionism

Callender and Cohen argue that there is "something wrong with the questions being asked about scientific representation" (Callender & Cohen 2006: 68). The fault lies with the assumption that there is something special about scientific representation that is in need of explanation. This is because, according to Callender and Cohen, a form of reductionism is the appropriate approach to scientific representation: in some important sense spelled out below, the vast majority of representations *reduce to* a privileged set of representations and there are no idiosyncratic or special questions about the former representations.

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[129] Phillips designed the model to represent the economy of the United Kingdom.
[130] There is, of course, debate as to whether or not economics ought to be considered a science: I'll bracket this here.

Callender and Cohen claim that the sorts of questions asked of scientific representation would be strange questions to ask of other kinds of representation. They ask:

> Are stop signs at intersections isomorphic or partially isomorphic to the imperative 'stop!' that they represent? Do they non-logically entail more true propositions than false ones? […] [D]o the marks 'cat' in any way resemble real cats? Are philosophers of language worried that the marks 'cat' aren't furry or that cats lack constituents that are part of an alphabet? These questions about non-scientific representations strike us as bad ones, and we hope they strike you that way too. This suggests to us that there may well be something wrong with the questions being asked about scientific representation. (*ibid*: 68)

It is true that these are bad questions and it is true that there are parallels to these questions being asked in the literature on scientific representation. The above passage is unsuccessful, however, when understood as an argument for thinking that these latter questions are wrong-headed. The fact that a question is a silly one to ask about linguistic representation only licenses inference to the claim that the question is also a silly one to ask about scientific representation if we *already* have good reason to think that linguistic and scientific representation are so similar that there are no silly questions to ask about linguistic representation that are not also bad questions to ask about scientific representation. However, this is effectively the claim that there are no special, or distinctive, problems of scientific representation.

Instead, the force of the critique ought to be located in the substantive view about representation that Callender and Cohen offer. This is a view – which I will call reductionism - appears to have as its consequence that there are no special problems of scientific representation.[131] I will take reductionism to consist in two claims.

The first, which Callender and Cohen call General Griceanism, states that "among the many sorts of representational entities (cars, cakes, equations, etc.), the representational status of most of them is derivative from the representational status of a privileged core of representations" (*ibid:* 70). For Callender and Cohen, a representation is fundamental if its representational status is not explained by citing information about other representational

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131 Callender and Cohen's view has deflationary characteristics: however, Suárez refers to his view as deflationism about scientific representation (Suárez 2004), so it would muddle matters to refer to Callender and Cohen's view as 'deflationism'. Callender and Cohen's view *reduces* the problems of most kinds of representation *to* the problems of mental representation – hence the name 'reductionism'.

states *and* is used to explain the representational status of other representations. A representation is derivative if its representational status is explained by citing information about other representational states.[132] According to General Griceanism, almost all representations are derivative: including scientific representation. Callender and Cohen take mental states to be the fundamental bearers of representative content. This isn't a requirement of reductionism - nevertheless, if we accept the reductionist picture, mental states seem a good candidate for fundamental representation. Many prominent accounts of mental representation, for example, seek to explain (in a variety of ways) how mental states get their contents by appealing to facts about the causal relations that the states stand in (Dretske 1981; Devitt 1996; Millikan 1984; Papineau 1987) and *not* by facts about other representations.[133]

General Griceanism is silent when it comes to *how* derivative representations come to derive their representational status and content from some fundamental representation: it just says *that* they do. The second claim that I take to be constitutive of reductionism, then, is the following: stipulation is sufficient for a derivative representation to derive its content from a fundamental representation. In order to motivate this second claim, Callender and Cohen note that for any pair of objects *a* and *b*, it is possible for *a* to be a representation of *b* (Callender & Cohen 2006: 73). Derivative representation is, in some sense, cheap to come by. Callender and Cohen offer the example of linguistic representation. For Callender and Cohen, a linguistic representational vehicle represents in virtue of its utterer intending it to do so, and its utterer intending for the hearer to believe that it so represents. Disparate, yet also non-fundamental, representations can be equally reduced to mental representation:

[132] Callender and Cohen introduce two distinctions: the natural/non-natural distinction and the fundamental/derivative distinction. It is not stated explicitly how these relate, but I take it that the interaction between these distinctions should be understood as follows. A natural representation is one that has its representational status in virtue of some other non-representational state of affairs, or independently of mental states: the examples given are the rings of a tree representing its age and smoke representing fire. In order for a representation to be a fundamental representation it must be a natural representation (as being non-natural is inconsistent with being fundamental) but not all natural representations are fundamental representations. The rings of a tree are natural but are not fundamental because they do not explain the representational status of a derivative representation. Mental states are both natural *and* fundamental because their representational status is grounded in some non-representational fact *and* some non-derivative representations derive their representational status from them.

[133] This is complicated by the fact that a view of mental representation could suggest that mental representations get their content in virtue of some *other* mental representation.

> The upshot is that, once one has paid the admittedly hefty one-time fee of supplying a metaphysics of representation for mental states, further instances of representation become extremely cheap. (*ibid*: 74)

Beyond the observation about representation being easy to come by, there's no argument given for thinking that the mechanism by which derivative representations get their content is by stipulation. The view is certainly not *entailed* by General Griceanism. The claim about stipulation is an answer to the question: 'in virtue of what does a given derivative representation come to inherit the representational content of a mental representation'? General Griceanism is compatible with many other answers to this second question. The only constraint on permissible accounts to the nature of this derivation is that it respects the view that for any pair of objects *a* and *b*, it is possible for *a* to be a representation of *b*. Whilst stipulation seems sufficient for representation in some cases (consider, for example, proper name reference and initial baptisms), without doing more work in the philosophy of language, the claim about stipulation should be treated as one *possible* answer to the question about the mechanism connecting derivative and fundamental representations.

5.2.2. From reductionism to non-specialness

How does reductionism result in the view that there are no special problems of scientific representation? Recall that the representational capacity of a derivative, non-fundamental representation, is explained by appealing to the representational capacity of fundamental representations and then a substantive philosophical account is given of the representational capacity of "the fundamental bearers of content" (*ibid*: 73). Assuming that the fundamental bearers of content are mental states, the core work on representation is to be done in the philosophy of mind, deciding between rival accounts of mental representation. Outside of the philosophy of mind, the work to be done is merely to explain the relation between the representational capacity of a given derivative representation and the representational capacity of the relevant mental states.

It is clear to see how this picture of representation threatens to render work on representation in the philosophy of science surplus to requirements. Take, as an example, the Lotka-Volterra equations: a paradigm case of mathematical scientific representation. According to reductionism, there are two questions to answer. The first is 'in virtue of does the vehicle represent the target?' and Callender and Cohen's answer is via stipulation: stipulation is sufficient to activate a mental representation of the representational target. The second question is 'in virtue of what does the mental state represent the predator and prey population?'. Philosophy of mind, not philosophy of science, will tell us in virtue of what a particular mental state has this particular content. A scientific representational vehicle represents its target because the user intends it to and intends the audience to believe that it does. As Callender and Cohen say:

> [Scientific representation] is constituted in terms of a stipulation, together with an underlying theory of representation for mental states, isomorphism, similarity and inference generation are all idle wheels. (*ibid*: 78)

Accordingly, those pursuing questions about scientific representation, appealing to notions like similarity, isomorphism and inference generation, are in error. This error extends, I assume, to those pursuing questions about *mathematical* scientific representations.

5.3. Recovering some special problems

5.3.1. The argument for reductionism

Other than the comparison to analogous questions about linguistic representation discussed above, there is no explicit argument given in Callender and Cohen's paper.[134] The main reason given in favour of adopting General Griceanism is that it is "economical and natural" (*ibid*: 70) Given that much of the argument against the specialness of scientific representation relies on an endorsement of General Griceanism, much in turn relies on the considerations given in favour of this account of representation.

First, consider the claim that General Griceanism is economical. In one sense, this is surely true. The Gricean is required to provide an account of the fundamental form of representation (the "admittedly hefty one-time fee"), and then provides (roughly) the same account for all the non-fundamental forms – the account concerning their derivability from the fundamental form. A non-Gricean approach to representation must *also* provide an account of the fundamental form of representation, at some point, but must also provide several, local accounts. The non-Gricean cannot appeal to the representational capacity of

134 If General Griceanism is something like a methodological stance then the demand for an argument might be out of place.

mental states to explain non-mental forms of representation. However, once if we accept that this form of armchair economising is a plausible way of choosing which working hypotheses to adopt, the balance between economy and fruitfulness becomes very important. It is plausible that by addressing non-mental forms of representation more narrowly, more progress can be made – philosophers can solve smaller, easier (!) problems about this or that form of representation.[135] Of course, Callender and Cohen will avow that such progress is not progress at all, but this is a consequence *of* General Griceanism.

Consider the claim that it is "natural" to describe all kinds of representation as derivative from a fundamental form of representation. If it is natural at all to describe all kinds of representation as being derivative from a fundamental form, this stems from taking denotation to be all there is to representation. In order to illustrate the point that for any *a* and *b,* it is possible for *a* to represent *b*, Callender and Cohen give an example of a saltshaker representing Madagascar, where this representation relation is brought into existence by stipulation (*ibid*: 73). In this case, I agree that the saltshaker denotes Madagascar, and that creating conditions such that the saltshaker denotes Madagascar are trivial in the sense that Callender and Cohen discuss. There is good reason for thinking that Callender and Cohen take 'represent' and 'denote' to come to the same thing. For example, they say:

> [W]hether and how a model is *about something* shouldn't hang on this classification [of a discipline as a science]. (*ibid*: 71) (emphasis added)

And

> [S]cientists routinely use entities other than models – language, pictures, mental states, and so on – to represent the very same targets that models represent. (*ibid*: 71)

Consider also the examples they provide: the saltshaker representing Madagascar, the upturned left hand representing Michigan, a lantern in the window representing the coming of the British. In these cases, the function of the representational vehicle is just to denote the target: so long as the lantern in the window denotes the coming of the British, it can do its job of alerting others to this fact. If denotation is indeed the sole function of all vehicles,

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[135] Consider, also, that Callender and Cohen note that they do not give an argument for mental representation being the fundamental form of representation – it acts only as a placeholder. The Gricean approach, then, requires us to (a) identify the fundamental form of representation and then (b) provide an account of it.

and this function can be fulfilled in the way set out by way described by the combination of General Griceanism and the claim about stipulation, then Callender and Cohen's claim appears plausible – indeed, as they say, "natural". However, as I argue below (and indeed, as has been acknowledged by many commentators – see references below), many representations (including those used in science) have epistemic value and functions that are not accommodated by the mere fact that they denote, or refer to, their targets.[136] These generate special problems of scientific representation.

5.3.2. Some special problems of scientific representation

I have argued above that what argument is given for reductionism fails. In this section I argue that there are some special problems of scientific representation that (a) are not questions about the representational capacity of mental states (b) are not questions about the relationship between the representational capacity of derivative representations and fundamental representations and (c) are plausibly not questions the answers to which are determined wholly by the pragmatics of representational practices. This is consistent with holding that General Griceanism is correct about the relationship between derivative and fundamental representations.

Consider the Glasgow subway map above and the linguistic object 'the Clockwork Orange', used in an utterance in a context such that it refers to the Glasgow subway (rather than to the book or to a mechanical citrus fruit). In the sense of representation that is explained by reductionism, the map and the words represent the target equally and, as Callender and Cohen are at pains to emphasise, in much the same way. Both plausibly come to denote the target identically. Yet, even though the two representations are both are about, or denote, or refer to the subway system in the same way, the functions of the two vehicles are not the same. In short, a user can use the map to learn facts about the representational target, whilst this is not the case for the linguistic object.[137] As noted above, one can read off from the map information about the quickest way to a particular station from another station. None of these acts of surrogative reasoning – reasoning from facts about the vehicle to claims

[136] It does not seem uncharitable to understand Callender and Cohen as taking *denotation* as the subject of their Gricean analysis but to be referring (!) to it as "representation". The problem is that scientific representation involves *more than* denotation.

[137] Of course, once one learns that 'Molly' denotes a particular dog, a user can use the representation to learn one fact about the target: namely, that the name 'Molly' denotes it (in particular contexts of utterance). However, it is not the *function* of the name 'Molly' to facilitate learning about the target.

about the target[138] – can be performed by considering the words 'the Clockwork Orange' (although see fn.137). Return to the quote above, where Callender and Cohen note that scientists often use words and thoughts to represent the targets that are represented by their models. What all these representations share is that they all denote the same object – they are all representations *of* the same target. Yet, only the model can be used to learn about the target that is common across all the representations.

So: some representations can be used to learn about their target (like scientific models and maps) and some cannot – and there can be representations of the former and latter kind that share targets. Moreover, it seems that the *function* of the former kind of representation is to facilitate this kind of learning. The following are questions that naturally arise about representations like these – but crucially *not* of representations like 'the Clockwork Orange' and the thoughts of scientists. In virtue of what can users learn about representational targets by reasoning about the vehicles? What is it that makes some representations *good* epistemic tools for performing surrogative inference and some less good? What's the relationship between the similarity of vehicle and target and the extent to which the vehicle facilitates learning about the target? These are natural questions to ask about scientific representations and reductionism sheds no light on the answers to these questions, *even if* we accept its truth. Stating that scientific representations represent their target in virtue of the creator of the representation intending them to and the audience of the representation taking the vehicle to represent its target gets us no closer to answering these questions about the epistemic role of scientific representations. It seems similarly clear that a convincing account of mental representation equally sheds little light on these questions.

In line with the recent literature (Bolinska 2013; Bolinska 2016; Chakravartty 2010; Contessa 2007; Contessa 2011; Suárez 2010), call representations like the subway map and scientific representations *epistemic representations*. Scientific representations seem to be paradigmatic examples of epistemic representations. If the form of distinctively mathematical scientific representation being considered in the next chapter (and this thesis more generally) is a

[138] It would be infelicitous to say that users of the map infer from facts about the vehicle to *facts* (rather than to *claims*) about the target. Inference from vehicle-facts to target-facts takes place only when the map is a good one (or, in terms of the distinctions drawn in the next section, when the map is a (at least partially) faithful map). A surrogative inference to a false claim about the target is an act of surrogative inference all the same – it is just not an act of *sound* surrogative inference.

species of epistemic representation, it will be useful to discuss epistemic representation in more detail, which I do in §5.4.

It should be clear that the problem of epistemic representation is not applicable in all cases of representation: there is no puzzle about how we can infer from facts about the word 'cat' to facts about cats, in virtue of the fact that we generally can't (but see fn.137). It is true that the problem of epistemic representation applies to cases of representation that are not scientific: for example, we can infer from facts about the subway map to facts about the subway system and this seems to demand explanation, but the subway map is not a scientific representation. In this thesis I will not consider the question as to whether there are any deep features that distinguish scientific epistemic representations from non-scientific epistemic representations.[139] In this sense it is correct that there are no problems that are *unique* to scientific representations. However, there *are* questions about scientific representations that do not involve explaining how they derive their representational status from mental states.

5.3.4. Epistemic representation and pragmatics

So: scientific representations are a form of epistemic representations and there are questions about epistemic representation that are not trivial questions about how an epistemic representation derives its representational content from a fundamental representation. The work in the philosophy of science, dealing with the "idle wheels" (Callender & Cohen 2006: 76) of like isomorphism, similarity and inference generation should be understood as shedding light on this special property of scientific representations and we have *prima facie* reason to think that this area of research is in good standing.[140]

Callender and Cohen might note that they at no point denied that some representations are to be preferred over others: it is no part of their view that, if one wants to navigate the

[139] By 'deep features' here, I mean any features of the representations themselves that ground their status as either a scientific or non-scientific epistemic representation. The difference between scientific and non-scientific epistemic representation is *shallow* if it is grounded entirely by one's demarcation criteria for science. Everything I say here about epistemic representation and mathematical representation is consistent with both views.

[140] As I discuss later in this chapter, some of the philosophers discussed by Callender and Cohen present their views as views of what constitutes the representation relation and for others it is ambiguous exactly what aspect of the representational practice the structural similarity is supposed to account for. I agree with Callender and Cohen that it is a mistake to think that *the fact that the vehicle represents the target* is accommodated by the holding of some structural relation and I agree that it's at least plausible that some reasonably trivial notion like stipulation is sufficient to *make it such that* the model represents its target.

Glasgow subway system, a map from 1950 would be just as good as one from 2017, in virtue of the fact that they both successfully denote the subway system. Some representations, for some purposes, are better than others and this indeed may be because they are better tools for coming to learn about the target system. But these considerations, this response claims, are part of the pragmatics of our explanatory practices:

> [I]t should be clear that the constraints ruling out these choices of would-be representational vehicles are pragmatic in character: they are driven by the needs of the representation users, rather than by essential features of the artefacts themselves. (*ibid*: 76)

What is it for something to be part of the pragmatics of the explanation? There are two options. The first is that the facts that determine which subway map is a better representation, for the purposes of navigating the subway system, are determined entirely by properties and interests of the users of the representation and not by any facts about the representational vehicle. For obvious reasons, this can't be what makes it such that these are pragmatic questions. The *reasons for which* people prefer one representation over another will have to do with that user's preferences and goals, obviously: but what makes a representation preferable *once those purposes are chosen* are facts about the representational vehicle.

The correct claim, then, is that the facts that determine which subway map is a better representation, for the purposes of navigating the subway system, are determined *in part* by properties and interests of the users of the representation and so only partly by facts about the representational vehicle. So, on this picture, what makes one subway map better than another, or one representation of the atom better than another, is a combination of facts: some facts about the goals of a user and then some facts about the representational vehicle and target. The problem with the second version of the response is that Callender and Cohen and the vast majority of the philosophers engaging in the research program that they think is unwise agree on this point. Appeals to facts about the user of the representation are commonplace in the literature about scientific representation (Bueno & French 2011; Contessa 2007; Suárez 2003): we should understand discussions about epistemic representation as being discussions about what features of a representational target make it better for the purposes of learning about the target.

The situation is as follows. There are some questions that naturally arise from the use of scientific representations that are not questions about the representational capacity of mental states and are not questions about the relationship between the representational capacity of derivative representations and fundamental representations. Furthermore, whilst they are not *unique* to scientific representation, they are not questions that apply to the other derivative representations that Callender and Cohen list alongside scientific representation. Whether or not these questions about epistemic representation count as question about the pragmatics of our explanatory practices or not is primarily a question about where one draws this boundary. Regardless of whether or not questions about epistemic representation (and, therefore, questions about how the particularly *mathematical* character of mathematical representations affects their epistemic character) are part of the pragmatics of representation or not is orthogonal to whether or not they are questions worth addressing. Callender and Cohen suggest that, if we accept reductionism, there is "no longer any reason to think that there is a conflict between, say, Giere's similarity and Suárez's inference generation, and so no reason that there should be a dispute between proponents of such accounts" (Callender & Cohen: 78). But this does not follow. If we understand accounts of scientific representation as accounts of what makes vehicles epistemic representations (or what makes some vehicles better or worse epistemic representations), these accounts can still be understood as *rival* accounts of what property is responsible.

Callender and Cohen's reductionism does, however, generate the following requirement on plausible accounts of mathematical scientific representation. The relation between vehicle and target that makes it such that the vehicle denotes the target ought to be distinct from the relation between the vehicle and target that makes it such that the vehicle is an *epistemic* representation of the target. This is an important lesson to keep in mind and it will play a role in the discussion of mathematical representation in the next chapter. (Although, it could be pressed that this is a lesson that one can learn merely by reflecting on the fact that (for a weak meaning of 'represent' where it merely comes to mean 'denote') anything can be a representation of anything else and this is not an insight unique to Callender and Cohen's reductionism).

5.4. Epistemic representation

In the previous section I argued that epistemic representation generates problems that cannot easily be identified as the same problem as either (a) understanding fundamental representation or (b) understanding the relationship between fundamental and derivative representations. In this section, I set out Contessa's recent account of epistemic representation and explicate how it can be understood as answering these questions. I pay particular attention to the role played by structural relationships in Contessa's interpretational account of epistemic representation, as it is these relationships that play a crucial role in the accounts of mathematical representation discussed in the next chapter. In particular, structural relations between the vehicle and target should not be understood as securing either the vehicle's status as denoting the target, nor as securing the vehicle's status as an epistemic representation: rather, structural relations should be understood as fixing (what Contessa calls) the representation's faithfulness.

5.4.1. Epistemic and non-epistemic representation

Return to the subway map above, and contrast it with the linguistic object 'the Glasgow subway'. In the sense discussed above, only the former is an epistemic representation: only the map has the function of allowing a user to learn information about its target. Contessa characterises epistemic representation in the following way:

> A vehicle is an *epistemic representation* of a certain target for a certain user if and only if the user is able to perform valid (though not necessarily sound) surrogative inferences from the vehicle to the target. (Contessa 2007: 52)

Strictly speaking, the inferences are from *facts* about the vehicle to claims (or putative facts) about the target. An inference from a claim about a vehicle to a claim about the target is valid if it is licensed (in a sense discussed below) by the vehicle and an inference from a fact about a vehicle to a fact about a target is sound if it is a valid inference that has as its conclusion a true claim about the target. The inference to 'Hillhead station is next to Kelvinbridge station', using the map above, is both valid and sound. If the label 'Kelvinbridge' was swapped with the label 'Ibrox', resulting in a different map, then the inference to 'Hillhead station is next to Ibrox station' would be a valid but unsound inference.

5.4.2. Contessa's interpretational account of epistemic representation

In his 2007, Contessa sets out his interpretational account of epistemic representation. The notion of an interpretation is crucial to Contessa's account of surrogative inference:

"interpretation is what grounds both scientific representation and surrogative reasoning" (*ibid*: 51). Being able to reason about the target by reasoning about the vehicle is, for Contessa, symptomatic of having interpreted the vehicle in terms of the target.

It is useful to think of an interpretation is to think of the user as taking facts about the vehicle to stand for claims or putative facts about the target. Consider the subway map again: the conventions of the representational vehicle take each circle on the map to stand for a station, for lines between circles on the map to stand for tunnels between stations and so on. These conventions permit the translation of facts about the vehicle (*this* dot is three places away from *that* dot) into claims about the target (*this* station is three stops away from *that* station). Contessa offers what he calls an *analytic interpretation* as one particular kind of interpretation. The user identifies:

> A (nonempty) set of relevant objects in the vehicle ($\Omega^V = \{o^v{}_1, \ldots, o^V{}_n\}$) and a (nonempty) set of relevant objects in the target ($\Omega^T = \{o^T{}_1, \ldots, o^T{}_n\}$), a (possibly empty) set of relevant properties of and relations among objects in the vehicle ($P^V = \{{}^nR^V{}_1, \ldots, {}^nR^V{}_m\}$), where ${}^nR$ denotes an *n*-ary relation and properties are construed as 1-ary relations) and a set of relevant properties and relations among objects in the target ($P^T = \{{}^nR^T{}_1, \ldots, {}^nR^T{}_m\}$), and a set of relevant functions from $(\Omega^V)^n$ […] to Ω^V ($\Phi^V = \{{}^nF^V{}_1, \ldots, {}^nF^V{}_m$), where ${}^nF$ denotes an *n*-ary function and $(\Omega^V)^n$ is the Cartesian product of Ω^V with itself *n* times and a set of relevant functions from $(\Omega^T)^n$ to Ω^T ($\Phi^T = \{{}^nF^T{}_1, \ldots, {}^nF^T{}_m$) (*ibid*: 57).

Having identified the above, the user must take the representational vehicle (as a whole) to denote the representational target (as a whole). In addition to this denotation step, the user must *interpret* the target in terms of the vehicle in the following sense:

> 1. The user takes every object in Ω^V to denote one and only one object in Ω^T and every object in Ω^T to be denoted by one and only one object in Ω^V
>
> 2. The user takes every *n*-ary relation in P^V to denote one and only one relevant *n*-ary in P^T and every *n*-ary relation in P^T to be denote by one and only one *n*-ary relation in P^V
>
> 3. [The user] takes every *n*-ary function in Φ^V to denote one and only one *n*-ary function in ψ^T and every *n*-ary function in Φ^T to be denoted by one and only one *n*-ary function in ψ^V. (*ibid*: 58)

The aforementioned map/subway interpretation is analytic. Contessa also details a set of inference rules such that if an inference is drawn from the vehicle to the target according to one of the rules, the inference is valid – these are set out in appendix A.[141] It is important to note that Contessa's account entails that, much like representation *qua* denotation, epistemic representation is trivial to come by. This is grounded by the fact that Contessa's notion of interpretation is, essentially, a collection of denotation relations. Epistemic representation, then, can be achieved as easily as denotation can.[142] Consider Callender and Cohen's example of a salt shaker coming to represent, *qua* denotation, Madagascar. The salt shaker can just as easily come to be an epistemic representation of Madagascar. One might take each of the holes at the top of the salt shaker to denote one particular city in Madagascar, and take the relative distances and locations of the holes to denote the relative distances and locations of the cities of Madagascar. A user of the salt shaker, taken to be an epistemic representation of Madgascar, could perform inferences to claims about Madagascar from facts about the salt-shaker. Of course, unless one has a peculiar salt shaker, these are likely to be valid but unsound inferences: the epistemic representation may well be completely *unfaithful*. I discuss faithfulness in the next section.

5.4.3. The completeness and faithfulness of epistemic representation

It is clear that Contessa's account address some questions about representation (namely: in virtue of what are some representations ways of learning about their targets in a sense that other representations are not?) that are not addressed by an account of the derivation relation between fundamental representations or by an account of fundamental representation. A vehicle denoting its target is fixed by some act of the user. A vehicle coming to be an epistemic representation of its target is brought about by a user interpreting the target in terms of a vehicle.

So far, then, there is no space for structural relations between the vehicle and target: whatever the denotation relation is, it is not a morphism of some kind and both the initial establishing of a denotation relation between vehicle and target and coming to interpret the target in

[141] It's important to note that Contessa's notion of analytic interpretation of the vehicle in terms of the target is *not* the same as an isomorphism between vehicle and target, even though both involve one-one correspondences between parts of the vehicle and parts of the targets.
[142] I assume that denotation is easy to come by, although this is (strictly speaking) distinct from claiming (as Callender and Cohen do) that stipulation is sufficient to establish denotation (see §5.2.1).

terms of the vehicle involve only denotation.[143] In this section, I suggest a friendly modification to Contessa's account and then discuss the proper role that should be played by structural relations: that of grounding a representation's faithfulness. In the next section I discuss the fact that allocating this role to structural relations has the felicitous consequence of resulting in an account of epistemic representation that both allocates a crucial role to structural relations whilst avoiding influential criticisms of views that assign structural relations a role in explicating representation.

First, the friendly modification. Recall the user who, unwisely, takes the salt shaker not only to represent Madagascar *qua* denotation but to be an epistemic representation of Madagascar. This epistemic representation will yield valid inferences to claims about Madagascar, but these are likely to be inferences to false claims. Contessa is clear that the inferences permitted by a user's interpretation and the inference rules needn't always have as their conclusion a *true* claim about the target. The difference between the salt shaker and a map of Madagascar (where both are taken to be epistemic representations of Madagascar) is what Contessa calls faithfulness (*ibid*: 50). A vehicle represents a target *completely* faithfully only if all the valid inferences are sound. A completely faithful epistemic representation is a representation that *only* licenses sound inferences – inferences that have as their conclusion facts about the target. Correspondingly, a partially faithful epistemic representation licenses inference to *at least one* sound inference: at least one inference to a *fact* about the target.

It, of course, seems completely right that faithfulness comes in degrees: vehicles can be better or worse epistemic representations. Contessa's account, however, has the following counterintuitive consequence. Consider a detailed map of Madagascar, including information about the names and locations of Madagascar's cities, rivers and roads. Consider a second, simpler, map which only charts the locations and names of the cities of Madagascar. Imagine that all of the inferences licensed by the detailed map and the simpler map are *sound* inferences (that is, they are inferences to facts about Madagascar rather than to false claims about Madagascar). Therefore, both maps are equally faithful epistemic representations: indeed, they are both completely faithful epistemic representations because they license inference to zero false claims about their target. Yet, this will ring oddly in the ears of many: there is surely some sense in which the detailed map is more faithful!

143 The salt shaker will trivially bear some structural similarity with Madagascar, but it is certainly not *in virtue of* these structural similarities that it represents its target.

As it stands, Contessa's account cannot accommodate this. I suggest that this can be remedied by introducing a second axis along which we can locate epistemic representations. Let the *completeness* of an epistemic representation be determined by the number of facts about the target that the vehicle licenses inference to.[144] This captures our judgement that, even though the detailed map and the simple map are equally faithful (in Contessa's sense), the detailed map is a *better* representation in some sense: it is more complete, in that it licenses inference to a greater number of facts about the target. An up-to-date map of the Glasgow subway system is a completely faithful epistemic representation but it is not a complete epistemic representation. It is a fact about the subway system that Partick station was called Merkland Street until 1977, but inference to this fact is not licensed by the subway map. Nevertheless, we would refrain from saying that this makes the map less accurate, or less faithful. It does, however, make it less *complete*.

The faithfulness and completeness of an epistemic representation will be reasonably independent. A complete epistemic representation may be only partially faithful, in cases where the representation licenses inference to some false claims about the target in addition to licensing inference to all the facts about the target and, as just noted, almost all completely faithful epistemic representations will fail to be complete epistemic representations.[145] A third dimension along which we may evaluate an epistemic representation is, of course, its usefulness. Whilst the usefulness of an epistemic representation will plausibly track its faithfulness (if only in the minimal sense that a very unfaithful epistemic representation is unlikely to be useful), it will less closely track its completeness: for an epistemic representation to be maximally useful, there will almost always be no need for the epistemic representation to be complete.[146]

That epistemic representation is binary and faithful epistemic representation comes in degrees is one important difference between the two. This yields a second important difference: that the conditions under which epistemic representation and (partially) faithful

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[144] One worry is that the completeness of an epistemic representation will be determined by how we carve up facts – both with regards to individuation of facts and distinguishing relevant from irrelevant facts. Perhaps we should understand completeness as being indexed to a particular subset of the facts.

[145] Indeed, every scientific representation will be a (at least partially) faithful epistemic representation rather than a complete epistemic representation.

[146] What makes an epistemic representation useful will, clearly, be determined by the users of the representation and their goals. Beyond this, there is plausibly little that can be helpfully said about usefulness that would not take this thesis too far afield.

epistemic representation are achieved are distinct (of course, the conditions under which (partially) faithful epistemic representation is achieved will include all the conditions under which epistemic representation is achieved). The conditions for epistemic representation, according to Contessa, are that the vehicle is taken to denote the target (brought about via stipulation, or some more complex user-initiated activity) and that the user interprets the vehicle in terms of the target (which is, again, just the establishing of more denotation relations). However, these conditions being met does not guarantee that the epistemic representation will be at all faithful, or accurate, – a subway map with swapped around labels will count as an epistemic representation if an interpretation is adopted, but it will be a very (or perhaps totally) unfaithful epistemic representation.

So, what makes an epistemic representation faithful? The fact that the two kinds of denotation relation involved in representation and epistemic representation do not suffice for the faithfulness means that something more sophisticated is required. Contessa notes when referring to Suárez's arguments discussed below that a natural answer is to gesture towards similarity between the target and the vehicle (*ibid*: 50).[147] Why is a subway map with swapped-labels less accurate, and therefore less useful, than the subway map above? That is, why does it license inference to fewer facts about the subway system? On the map above, the object on the map takes to denote Hillhead is next to the object on the map that the user takes to denote Kelvinbridge – whilst, on the swapped-map, the same objects on the map fail to be next to each other. I take it that this *structural* account of faithfulness adds the following component to Contessa's remarks above.

> 4. There is a structure-preserving mapping between the vehicle and target that is responsible for the soundness of (at least some of) the inferences licensed by the denotation relations (1-3) and the rules of inference.

Recall, once more, that stipulation (or some other user-fixed input) secures a vehicle's status as a representation *qua* denotation, the various denotation relations set out in (1), (2) and (3) jointly secures a vehicle's status as an *epistemic* representation and (4) secures a vehicle's status as a (partially) faithful epistemic representation. Many of the substantive questions about the kind of structural account suggested in this section will raise their heads when accounts of

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[147] Contessa says: "insofar as [similarity accounts of representation] can be interpreted as either accounts of scientific representation or accounts of faithful scientific representation, they would seem to be accounts of faithful scientific representation" (Contessa 2007: 50) (*cf.* Contessa 2011: 127-130).

mathematical scientific representation are considered in the next chapter – so beyond a brief discussion in the next section, I will delay discussion until then.

5.4.4. Faithfulness and the role of structural relations in epistemic representation

The suggestion above that it is structural relations between the vehicle and the target that secure the (partial) faithfulness of an epistemic representation should bring to mind claims made in the literature on scientific representation that representation, in some sense, depends on the structure of the vehicle and target. In this section I briefly distinguish the role played by structural relations in the account above, and the role (purportedly) played by structure in these accounts. This is important – that structural relations play the role of determining the faithfulness of an epistemic representation is vital in responding to the Suárez-Frigg objections, which take aim at accounts of representation that incorporate the notion of similarity.

Chakravartty's distinction between functional and informational accounts of representation can help shed light on the different roles that structural similarity can play in an account of representation (Chakravartty 2010). According to an informational account of scientific representation, what *makes it such that* a representational vehicle represents its target is that some form of relation holds between the target and vehicle (Chakravartty 2010: 198). Putative examples include Ronald Giere's similarity account (Giere 2004), Bartels' account that focuses on homomorphisms (Bartels 2006), and the work of da Costa and French involving partial isomorphisms (da Costa & French 2003).[148] According to functional accounts of representation, as one might expect, representations ought to be distinguished from non-representations by gesturing towards their functions: "their uses in cognitive activities performed by human agents in connection with their targets" (Chakravartty 2010: 199). On this distinction, Suárez's inferential account and Contessa's interpretational account of representation are functional accounts. After introducing this distinction, Chakravartty argues that the distinction is a false one: whilst functional accounts of representation should be understood as accounting for the ends of a representation, the informational accounts should be understood as the means.

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[148] I'm not certain that all of the philosophers that Chakravartty mentions make as flat-footed a claim as that which says that the existence of the candidate structural relation is solely constitutive of the representational relation. However, what's important here is to contrast such accounts (whether or not any of the examples Chakravartty provides count as such accounts) with what is being endorsed in this chapter.

What should we make of the structural informational accounts of scientific representation, in light of the above structural epistemic account of representation and in light of Chakravartty's dissolving of the tension between informational and functional accounts? One suggestion, made by but not fully endorsed by Chakravartty (*ibid*: 209-210) is that the proponents of the above structural accounts of representation may wish to repurpose their accounts as accounts of the structural relations that are responsible for the accuracy or faithfulness of a representation. However, it is yet to be seen as to whether or not the same structure-preserving mapping between the vehicle and target are responsible for the vehicle being (partially) faithful in every case of representation. There may be nothing fully general to be said about *which* structure-preserving mapping is responsible for (partially) faithful epistemic representation: it is a necessary condition on (partially) faithful epistemic representation that there is some structure-preserving mapping between vehicle and target, but this condition will be satisfied by different relations in different cases of representation. That there is *a* structure-preserving mapping between vehicle and structure is a fully general explanation for why a given epistemic representation is (partially) faithful but that there is an isomorphism (for example) between vehicle and structure will *not* be a fully general explanation (if, as it seems likely, there are cases of (partially) faithful epistemic representation where the relation between vehicle and target is weaker than isomorphism). I will return to this point when considering some more examples of mathematical scientific representations in the next chapter.

5.5. The Suárez-Frigg objections

According to the account endorsed above, for a given epistemic representation, some user-generated denotation relation makes it such that the vehicle represents the target, another set of user-generated denotation relations between objects, properties and relations in the vehicle and target make it such that the vehicle is an epistemic representation of the target and structural similarity between the vehicle and target makes it such that the vehicle is a (potential) partially faithful epistemic representation of the target.[149] In the next chapter, I argue that mathematical representation can be understood as a kind of (partially) faithful

[149] Structural similarity suffices only for the *potential* of being a partially faithful epistemic representation: without a user bringing it about that the vehicle represents the target and taking (relevant) objects and properties in the target to be denoted by objects and properties in the vehicle, a vehicle being structurally similar to a potential target does not suffice for being an epistemic representation.

epistemic representation. Part of what makes mathematical representation amenable to treatment using the epistemic representation framework is the role given to structural relations.

However, one of the lessons of the recent literature on scientific representation is supposed to be that building the notion of structural similarity into one's account of representation leaves the account open to obvious objections. This stems from arguments owing to Suárez (Suárez 1999; Suárez 2003) and Frigg (Frigg 2002; Frigg 2006). This poses the same kind of fundamental challenge to the project of the next chapter as Callendar and Cohen's objection discussed in §5.2: if Suárez and Frigg are correct, then *any* account of mathematical representation appealing to structural relations is mistaken. This is especially relevant, given that Pincock's account of mathematical representation discussed in the next chapter is often described as being a version of the structural account of scientific representation (for example, Pero & Suárez 2016) and so is presumably threatened by these objections.[150] In this section I discuss these objections and demonstrate that the account of representation endorsed here avoids them.

Suárez takes his arguments to be targeting the following views:

> [sim]: A represents B if and only if A is similar to B.
>
> [iso]: A represents B if and only if the structure exemplified by A is isomorphic to the structure exemplified by B. (Suárez 2003: 227)

Suárez accepts that isomorphism (and so presumably other kinds of structural mappings) is a kind of similarity, noting that "it is possible in general to understand isomorphism as a form of similarity" (*ibid*: 228). It is harmless to take the view that Suárez as targeting as being the following, where being structurally similar is understood as there being some morphism between A and B.[151]

> [sim-iso] A represents B if and only if A is (structurally or non-structurally) similar to B.

[150] In §6.4 I argue that this is mistaken.

[151] Suárez originally takes aim at structural accounts of scientific representation that appeal to isomorphisms, but later levels similar objections to views that appeal to other morphisms (Pero & Suárez 2016).

Suárez then proceeds to present a series of arguments against this view. I will briefly set them out below before demonstrating that the view endorsed in the preceding section is not susceptible to them.

5.5.1. The arguments

According to the logical argument, [sim-iso] fails to respect the logical properties of representation.[152] Representation, as Suárez notes, "is an essentially non-symmetric phenomenon […] Representation is also non-transitive and non-reflexive" (*ibid*: 232). However, if A and B are similar, then A is similar to A and B is similar to A, and if A and B are isomorphic then A is isomorphic with A and B is isomorphic with A. This is in tension with our judgements that, for example, the hydraulic machine represents the economy but it is not the case either that the hydraulic machine represents itself or that the economy represents the hydraulic machine.

According to the argument from misrepresentation (*ibid*: 233), [sim-iso] entails that particular vehicles are representations of particular targets, even though we judge this to be false. To adapt Suárez's example: a painting of Homer is not a representation of Homer's brother Herbert, even though Herbert is incredibly similar to Homer (and therefore to the painting). There fails to be a representation relation between the painting and Herbert, but "there is no failure of similarity to explain it" (*ibid*: 234).[153] Suárez continues to note that [sim-iso] has problems in dealing with the fact the accuracy (or in the terms used above, faithfulness) of a representation comes in degrees.[154] Suárez argues that views appealing to isomorphisms fail to account for the gradable nature of faithfulness, given that whether or not a vehicle is isomorphic to (the structure of) its target is binary but accuracy is not.

According to the non-necessity argument (*ibid*: 235), there can be a representation relation between a vehicle and a target in the absence of a relation of (structural) similarity. Suárez introduces Picasso's *Guernica* and an arbitrary equation as examples. *Guernica* represents both the bombing of Guernica and the looming threat of fascism (*ibid*: 236): yet if the painting is

152 Goodman (Goodman 1976) offers an early version of the logical argument, as understood as targeting similarity accounts of representation in the philosophy of art.
153 This is much the same point as made by Putnam and his discussion of the ant who, by chance, traces a shape in the sand that resembles Churchill (Putnam 1981).
154 Suárez refers to both of these problems as 'the argument from misrepresentation'.

similar to either of these representational targets, it is only in the reasonably trivial sense in which it is equally similar to many other things that it fails to represent. Similarly, Suárez claims, Newtonian mechanics represents the solar system even though it is "clear that Newtonian mechanics, without general relativistic corrections, is empirically inadequate and non-isomorphic" to planetary motion (*ibid*: 236).

As one might imagine, Suárez's non-sufficiency argument claims that there can be a relation of (structural) similarity between a putative vehicle and target without it being the case that the vehicle in fact represents the target (*ibid*: 236). There are many pairs of objects that are similar to each other, without it being the case that either represents the other: similarly, Suárez describes a trajectory in phase space that (without us knowing) is isomorphic to the motion in physical space of a classical particle. Despite the relation of isomorphism holding between the phase space model and the motion of the particle, we judge that the model fails to represent the particle.

5.5.2 Responding to the arguments

One possible way to defend a broadly structural view of representation is to specify the kind of (structural) similarity in more detail: for example, opting for morphisms other than isomorphism restores the parity of at least some properties and thereby hopefully avoiding the logical objection, at least. Bartels (Bartels 2006) takes this route, opting for homomorphism. Similarly, appealing to partial isomorphism restores the properties of asymmetry and non-transitivity (Bueno & French 2012: 887). (Although see fn.178 for an important caveat regarding the partial structures framework that distinguishes it somewhat from Bartels' suggestion). Further, one might press that proponents of the views that Suárez and Frigg are objecting (taking (structural) similarity to be necessary and sufficient for representation) might be harder to find than they think.[155]

There are some *prima facie* reasons for not endorsing this strategy of specifying the structural relation in more detail. The first concerns the details of such proposals. As Pero and Suárez note in their response to Bartels, the homomorphism structural account still requires

[155] There are also passages in which proponents of structural views *do* sound like they think (structural) similarity is *sufficient* for representation. One tangible benefit of Suárez's contribution to this literature is the emphasis on being much clearer on exactly what aspect of our representational practices a particular account is being offered to accommodate.

appealing to non-structural features to ground some features of representation (Pero & Suárez 2016). Similarly, it is not clear that the *logical objection* can be responded to merely by specifying the relevant structural relation in detail: whilst homomorphism and the morphisms of the partial structures framework retain the asymmetry of representation, they are nevertheless reflexive. Furthermore, both specifying the structural relation in more detail and reiterating that the holding of such a relation is not sufficient for representation will do nothing to respond to the purported cases of representation in the absence of such relations (the *non-necessity argument*). I think, though, what the above discussion of Contessa's account reveals is that it is a mistake to think what a structural relation between vehicle and target does in cases of representation is to make it such that the vehicle represents the target (what Suárez calls the *constituents* of representation): this is what goes wrong with these possible responses to the Suárez-Frigg objections. (This is not to say, of course, that the defender of Bartels' homomorphism account (for example) could not do more than gestured at here to respond to the Suárez-Frigg objections – the below is an *alternate* route).

Once the role of (structural) similarity is shifted to grounding the faithfulness of an epistemic representation, these arguments lose their force. The account endorsed here avoids the logical objection, in virtue of the fact that the denotation relations responsible for the fact that the vehicle denotes the target and that the objects/properties/relations in the vehicle denote the objects/properties/relations in the target restore the directionality of (epistemic) representation: the same goes for the argument from misrepresentation. The non-sufficiency and non-necessity objection is avoided merely in virtue of the fact that the structural epistemic representation account does not state that the holding of a structural relation between vehicle and target is sufficient for either representation *qua* denotation nor for epistemic representation. *Guernica*, the proponent of the epistemic representation account should claim, is *about* the bombing of Guernica and is *about* the rise of fascism but it is not an epistemic representation of either of these things.

It is, of course, unsurprising that Contessa's account, descended from Suárez's own deflationist account of representation, should have properties that allow it to avoid the objections that Suárez raises! Contessa takes himself to be building on Suárez's account (Contessa 2011: 51) and this, of course, is the reason that the above responses to the Frigg-Suárez objections fall out so easily from the account. This is not novel, but it *is* important. Indeed, it is a widely appreciated feature of Contessa's approach: Bolinska notes that adopting an epistemic representation account "avoids some of the objections that have been raised in response to accounts that appeal to some sort of structural similarity between vehicle and target system" (Bolinska 2013: 220), referring to the Frigg-Suárez objections. However, she problematically goes on to say that "without relying on structural relationships between vehicle and target [the epistemic representation approach] manages to say something substantive about scientific representation" (*ibid*: 221). This can't be quite right. As noted above, taking an epistemic representation approach *should* rely on structural relationships but it should *not* do so in grounding the fact that the vehicle denotes the target or is an epistemic (rather than non-epistemic) representation. Proponents of the epistemic account should not (as is sometimes done) understand the account as eschewing structural similarity, but rather putting it in its proper place.

Before concluding, consider the following recent passage from Pero and Suárez, in which they seem to make a similar point as Chakravartty makes and that I have been making in this section:

> It is in particular often unclear […] whether isomorphism and its cousins are intended to provide an analysis of the notion of representation itself, or whether they are merely intended to describe some of the ways in which representations in science achieves some of its characteristic ends, such as for instance, the aim of accuracy. […] Defenders of the structural accounts are often imprecise in shifting from evidence for the weaker case to claims in favour of the stronger constitutive claim. (Pero & Suárez 2016: 57)

I take this to be broadly correct and one of the aims of this chapter has been to make precise exactly what role should be played by structural relations. However, the distinction between structural relations being part of "an analysis of the notion of representation itself" or being invoked only to explain how we achieve some of our goals using representations is a false dichotomy. The view endorsed here says that structural relations are not part of the analysis of representation *qua* denotation, but they *are* part of the analysis of faithful epistemic representation. Recall that Contessa justifies adding the interpretation condition to Suárez's earlier inferential account of representation on the basis that not doing so makes our ability to perform inferences from vehicle to target mysterious.[156] The same sort of argument

[156] Suárez's inferential account states that A represents B only if (i) the representational force of A points towards B and (ii) A allows competent and informed agents to draw specific inferences regarding B (Suárez 2004). Both Contessa and Suárez's views, then, place surrogative inference at the heart of epistemic representation: it's important to briefly clarify in what sense the views are distinct. Suárez demurs from placing

justifies adding the necessary condition that the vehicle stands in a structural relation to the target: without this addition, it is left unexplained how it is that a user is able to infer to *facts* about the target.[157] The best explanation of the (partial) faithfulness of epistemic representations is that the vehicle is structurally similar to the target and this explanation is very plausibly fully general.

5.6. Conclusion

In this chapter I considered two recent claims about scientific representation that threaten the narrower project of the next chapter. The first claim, Callender and Cohen's reductionism about scientific representation, is that there are no questions about scientific representation that aren't understood as questions about how mental representations get their content or about how derivative representations inherit their representational status. In response, I argued that there are functional differences between derivative representations and that how epistemic representations perform their role is a question left unanswered by Callender and Cohen's reductionism. I then set out Contessa's account of epistemic representation: I suggested that such representations can be compared regarding their completeness as well as their faithfulness and that structural relations ought to be invoked to ground an epistemic representation's degree of faithfulness. This account is not threatened by the Suárez-Frigg objections. In the next chapter, when considering Colyvan & Bueno and Pincock's proposed accounts of distinctively mathematical scientific representation, one of the questions I investigate is whether or not these accounts give structural relations the kind of role that avoids the Suárez-Frigg objections.

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any other sufficient condition on epistemic representation beyond denotational force and surrogative inference. Contessa (and following him, Bolinska (Bolinska 2013)) argues that this "leaves one with the mistaken impression that there is something mysterious about our ability to use models to perform pieces of surrogative reasoning about their target systems" (Contessa 2007: 51). There is a sense in which this objection is not quite right: Suárez's view is not that we should mysterians about any question of the form "for this particular given epistemic representation, in virtue of what features of the user, vehicle and target can the user perform surrogative inferences from facts about the vehicle to (putative) facts about the target?". He admits that in particular cases, there will be some set of features with which we can explain the user's ability to perform sound surrogative inferences: "in every specific context of inquiry, given a putative target and source, some stronger condition will typically be met; but which one specifically will vary from case to case. In some cases it will be isomorphism, in other cases it will be similarity, etc." (Suárez 2004: 776). Contessa takes himself to be supplying an explanation as to why a user can provide surrogative inferences. Suárez is correct that there is no one-size-fits-all answer to the question 'what particular relation between vehicle and target grounds the vehicle's degree of faithfulness?'. What is illicit is the inference from 'there is no relation between vehicle and target that grounds the degree of faithfulness of an epistemic representation, in all cases' to 'there are no necessary conditions on epistemic representation beyond denotation and the ability to perform surrogative inference'.

[157] It's important to reiterate that structural similarity between vehicle and target is a necessary condition on faithful epistemic representation and *not* on either denotation nor epistemic representation *simpliciter*.

Chapter 6

Distinctively mathematical epistemic representation

The previous chapter attended to two issues concerning scientific representation *simpliciter*: the first concerned whether or not there are any distinctive, or special, problems about scientific representation and the second concerned what role structural similarity ought to play in one's account of representation. This chapter is about distinctively mathematical scientific representation. A quirk of the recent literatures on scientific representation and the applicability of mathematics is that there is relatively little dialogue between the two. Much of the discussion of mathematical scientific representation goes under the name of the applicability of mathematics: but what constitutes the applicability of mathematics, if not the construction and utility of distinctively mathematical scientific representations?[158] 'Scientific representation' and 'mathematical scientific representation' are not co-extensional, but the latter *is* a subset of the former. The Lotka-Volterra equations, for example, should be accommodated both by one's general account of scientific representation and by one's account of mathematical scientific representation given they exist at the intersection of these two categories.

What could explain the lack of explicit dialogue? Perhaps it is *assumed* that the best theory of scientific representation will be straightforwardly compatible with the best theory of mathematical scientific representation. Although in this chapter I argue that we can understand mathematical scientific representation using the epistemic representation framework, this is not a trivial matter and there is work to be done. Furthermore, the little that has been said about the relationship between accounts of scientific representation *simpliciter* and accounts of mathematical representation seems to misunderstand one or the other. This chapter, then, does not *instigate* the task of thinking about how mathematical applicability and general theories of scientific representation relate to each other. It does,

[158] See §0.4 for comments about the relationship between offering an account of the representational capacity of mathematics and offering a solution to the problem of the unreasonable effectiveness of mathematics.

however, provide a corrective to what has been said so far in the literature and suggest another way of approaching the question.

In section one I set out some examples of mathematical scientific representations. In section two I set out the details of Pincock's mapping account. In section three I set out the details of Bueno and Colyvan's inferential conception, which is presented as an improvement over the mapping account. In section four I argue against a recent way of understanding how these accounts relate to accounts of scientific representation *simpliciter*, suggested by Pero and Suárez. I argue that deficiencies with this proposal naturally point in a different direction: that of demonstrating the mapping account and inferential conception are consistent with Contessa's epistemic framework, which I do in section five. In section six, I canvas the reasons one might have for thinking that the mapping account and the inferential conception are in tension and argue that there is substantive agreement between the accounts. In section seven I set out what I take to be the common core of the two accounts and work through an example. This common core can be accommodated by the epistemic representation framework and is what provides the answer to the question 'in virtue of what does a piece of mathematics (epistemically) represent something non-mathematical'? It is with this core in mind that the debates about the plausibility of representationalist nominalism ought to be addressed. Accordingly, in section eight, more tentatively, I discuss the metaphysical commitments of the account endorsed. I evaluate Bueno and Colyvan's claim that their account is metaphysically neutral and Pincock's claim that in order to understand the content of a mathematical representation, it is necessary to hold that the mathematical content of the representation is true.[159]

6.1. Examples of mathematical representations

Here is the first example, familiar from the previous chapter. Consider the Lotka-Volterra equations:

(1) $\frac{dV}{dt} = rV - (aV)P$

[159] Both of the two accounts I discuss here, and the common core that I suggest they share, give a crucial role to structural relations between the mathematical vehicle and the (assumed structure of the) physical target. Many other commentators suggest that they, too, believe that the representational power of mathematics is grounded (in some sense) by structural relations between the mathematical and physical domains (Azzouni 2004; Balaguer 1998; Baker 2003; Leng 2002; Shapiro 1997).

(2) $\frac{dP}{dT} = b(aV)P - mP$

What is being represented is either some particular predator-prey population, or predator-prey populations in general. (I give more detail about the Lotka-Volterra example in §5.1 and §6.7.2). The first distinctive feature is that this is a case both of representation *qua* denotation and *qua* epistemic representation. The representation both denotes the predator-prey population and can be used to learn facts about it. The second distinctive feature is that this is an example of scientific epistemic representation: the equations are used in various scientific contexts to represent various target systems of scientific interest.

Here is a second example of mathematical representation, drawn from a textbook on differential equations used in engineering (Xie 2010).[160] Take the bridge below.

Image removed due to copyright.

Figure 3. Suspension bridge

The origin *O* is set at the lowest point of the cable, understanding the cable to be "modelled as subjected to a distributed load *w*(*x*)" (Xie 2010: 4). The shape of the cable is given by the following second-order ordinary differential equation, taking H to be the tension in the cable at the lowest point O.

[160] My presentation of this simple case follows Xie's: see Xie 2010 §1.2 for more examples. The uses of differential equations in engineering are not exhausted by examples like the above, of course, nor are the uses of representational mathematics outside of scientific practice limited to engineering.

(3) $$\frac{d^2y}{dx^2} = \frac{w(x)}{H}$$

Just as with the Lotka-Volterra case, this is an example of both representation *qua* denotation and (at least partially) faithful epistemic representation.[161] Furthermore, just as with the Lotka-Volterra case, this is an example of distinctively *mathematical* epistemic representation. However, unlike the Lotka-Volterra equations, the suspension bridge equation is not straightforwardly a case of *scientific* representation. As in the previous chapter, when drawing a distinction between the Lotka-Volterra equations and the subway map, in this thesis I will not defend a particular account of the demarcation between science and non-science: no account is offered of what makes it the case that the context in which the above representation is deployed is non-scientific and the context in which the Lotka-Volterra equations are deployed is scientific. Nevertheless, I will take it that there is nothing intrinsic about the representations that make some scientific and some non-scientific.[162]

6.2. The mapping account

In this section, I set out Pincock's mapping account of mathematical representation. Although it is discussed in earlier papers, (Pincock 2004a; Pincock 2004b; Pincock 2007) Pincock's account is set out in the most detail in *Mathematics and Scientific Representation* (Pincock 2012). I will accordingly refer back to Pincock's earlier discussions when it is required to disambiguate aspects of his mature view. In this section I attempt to set out the core of Pincock's account, focusing on aspects that are relevant to the later comparisons both with Bueno & Colyvan's account and Contessa's account of epistemic representation. I cannot attend to the richness of Pincock's discussion in this space: recall, though, that what we're trying to get at is *in virtue of what* a piece of mathematics represents something non-mathematical (both epistemically and *qua* denotation). Further details will be given as and when required (for example, in the discussion in §6.6.1. of how the accounts understand idealizations).

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[161] The same questions arise concerning exactly what the representational target is in in any given use of the equation: I return to this point when working through the Lotka-Volterra case in §6.7.2.

[162] There is a sense, then, in which the question as to whether or not a given epistemic representation counts as a *scientific* representation is being off-loaded to those philosophers of science working in the viability of a demarcation criterion. Everything said about these representations here is consistent with there being *no* viable demarcation criterion, with the relevant indicators being either descriptive or normative, or (I take it) some other view. For a good recent overview, see part 1 of Pigliucci and Boudry 2013.

An important initial distinction, on Pincock's account, is between a model and a representation (Pincock 2012: 26) – a model, for Pincock, is "any entity that is used to represent a target system" (*ibid*: 26) whilst a representation is a "model with content" (*ibid*: 26). This allows for a natural understanding of how (for example) the Lotka-Volterra equations can be used to represent different target systems on different occasions: in Pincock's vocabulary, then, the same model is used across different representational contexts: the model is the same but the representation is different because the model has different contents in different contexts.[163] For Pincock, contents "provide conditions under which the representation is accurate": offering a mathematical scientific representation is to claim that the "concrete system S stands in the structural relation M to the mathematical system S*". A representation is correct if "both systems exist and the structural relation obtains" (*ibid*: 28).[164] Pincock states that, for any mathematical representation, we can identify its content by asking the following questions:

1. What mathematical entities and relations are in question?
2. What concrete entities and relations are in question?[165]
3. What structural relation must obtain between the two systems for the representation to be correct? (*ibid:* 27)

The notion of a structural mapping between vehicle and target plays a crucial role, hence the name of the mapping account. It is worthwhile briefly making clear what is meant by a structure in this context. The standard notion of structure takes a structure $\mathcal{S}$ to be a pair,

[163] As discussed in the previous chapter when considering whether or not the 3D hydraulic model and the subway map (for example) are representations in the same sense, there is ambiguity as to the relationship between models and representations. There is a natural way of using the two terms in which models are built from representations and, as such, representational vehicles are more basic. However, on Pincock's use of the two terms, all representations are models but not all models are representations – a model becomes a representation once it has been given some content. Investigating the relationship between models and representations, and whether there is any principled way of using the two terms in which they come apart, is beyond the scope of this thesis. I will continue to use the terms vehicle and target (even though Pincock and Bueno & Colyvan do not use these terms) in order to remain as neutral as possible.

[164] It is important to note what seems to be a tension in two parts of Pincock's account. Pincock assumes the truth of mathematical realism when setting out the details of his account (hence the talk of "both systems exist[ing]"), but walks this back when discussing what he thinks the metaphysical commitments of the account are. As is discussed in §6.8.2, Pincock holds that the account only requires truth-value realism: upon making this clear, he does not offer a restatement of the accuracy conditions. These two aspects of Pincock's account seem, therefore, to be in tension.

[165] Pincock's use of the term "in question" seems infelicitous – I take it that what he means is, we should be able to answer the question "what are the mathematical and concrete objects and relations involved in the representation?".

consisting of a set of objects D of the structure (or the domain or universe of the structure) and a set of relations extensionally defined on D.[166]

6.2.1. Structural relations and specification

In addition to a structural relation between vehicle and target, the mapping account involves another kind of relation, which is logically prior to the structural relation. What Pincock calls the *specification* helps inform the content of the vehicle, and includes some form of interpretation of the mathematics in terms of the target. Pincock introduces the idea of a specification as follows:

> The conditions of correctness that such representations impose on a system can be explained in terms of a formal network of relations that obtains in the system along with *a specification of which physical properties are correlated with which parts of the mathematics*. (*ibid*: 25) (emphasis added)
>
> Suppose we have a concrete system *along with a specification of the relevant physical properties. This specification fixes an associated structure.* Following Suárez, we can say that the system instantiates that structure, relevant to that specification, and allow that structural relations are preserved by this instantiation relation. This allows us to say that a structural relation obtains between a concrete system and an abstract structure. (*ibid*: 29) (emphasis added)

These quotes make clear that the specification plays two roles: it provides an interpretation of the physical structure and it selects a mathematical structure with which to denote the target system (or, in Pincock's vocabulary, it selects a model). A physical system is taken to instantiate a particular structure, which facilitates the standing of the system in structural relations with the mathematical structure. Some parts of the vehicle are "assigned denotation or reference relations" (*ibid*: 257) – I assume what the parts are taken to denote/refer to are objects (or properties or relations) in the physical system.

Pincock offers the following, typical, definition of a structural relation, which coheres with that given above:

[166] Below, I give an example of a structural relation that is stated using mathematics but, obviously, once mathematically-stated structural relations are permitted it is impossible to provide an exhaustive list.

> A structural relation is one that obtains between systems S1 and S2 solely in virtue of the formal network of the relations that obtains between the constituents of S1 and the formal network of the relations that obtains between the constituents of S2, [where a] formal network is a network that can be correctly described without mentioning the specific relations which make up the network. (*ibid*: 27)

Pincock mentions isomorphisms and homomorphisms as potential structural relations that might play this role in particular mathematical representations (*ibid:* 27-30). In addition, Pincock permits incorporating mathematical notions in the structural relations: "including those whose specification requires mathematics" (*ibid*: 27) Pincock expands his discussion of content, to distinguish between several (more sophisticated) kinds of content that can be had by a mathematical representation. I will return to some of this when considering the mapping account in relation to the inferential conception in §6.6.1.

6.3. The inferential conception

Motivated by what they take to be deficiencies in Pincock's account (as set out in Pincock 2004), Bueno and Colyvan have recently offered an alternate account, the inferential conception. Bueno and Colyvan's account take as the motivating insight the fact that "the fundamental role of applied mathematics is inferential":

> [B]y embedding certain features of the empirical world into a mathematical structure, it is possible to obtain inferences that would otherwise be extraordinarily hard (if not impossible) to obtain. (Bueno & Colyvan 2011: 352)

The claim that facilitating inference is the "fundamental" role of applied mathematics should remind us of the discussion in §1.4.2 about the various roles played by mathematics and whether or not any of these depend on each other in substantive ways. Although Bueno & Colyvan state that inference is the fundamental role played by mathematics in science, this need not stand in tension with the suggestion that representation is (in some sense) basic and grounds the various other uses (such as inference and confirmation) that mathematics is put to: this is because, for Bueno & Colyvan, inference is grounded by mathematics' representational capacity in virtue of the fact that the notion of inference plays a crucial part in *explicating* mathematics' representational role. Following Hughes' DDI account of scientific representation (Hughes 1997), for Bueno & Colyvan is (partially) explicated by appealing to inference.

It is clear that by 'embedding', Bueno and Colyvan mean that mappings are established between the target system and the mathematical vehicle. So, like the mapping account, the inferential conception relies (in some sense) on structural relations that hold between the vehicle and target. Bueno and Colyvan engage directly with a worry that is mentioned in passing by Pincock: physical target systems don't seem to be the right kind of thing to stand in structural relations with mathematical structures, as they are neither set-theoretic nor mathematical objects.[167] Bueno and Colyvan's response is to assume that:

> [T]here is some natural structure of the [target system] or that an appropriate structure can be imposed upon the [target system] (*ibid*: 372)

This assumption about the structure of the target system is part of what Bueno and Colyvan call the "empirical set up" of a mathematical scientific representation.[168] With the empirical set up in hand, the inferential conception takes there to be three steps involved in making use of a mathematical scientific representation. Bueno and Colyvan set them out as follows:

> [T]he [immersion] step consists in establishing a mapping from the empirical set up to a convenient mathematical structure. We call this step *immersion*. The point of immersion is to relate the relevant aspects of the empirical situation with the appropriate mathematical context. […] [S]everal mappings can do the job here, and the choice of mapping is a contextual matter, largely dependent on the particular details of the application.
>
> [T]he [derivation] step consists in drawing consequences from the mathematical formalism, using the mathematical structure obtained in the immersion step. We call this step *derivation*. This is, of course, the key point of the application process, where consequences from the mathematical formalism are generated/

167 Pincock is similarly noncommittal about how we should think about the target system having a particular structure: he says that there is "an important difference between talking about a concrete system made up of objects and linked together by concrete relations involving quantities and properties and a set-theoretic structure" (Pincock 2012: 29).

168 It is also important to briefly distinguish Bueno & Colyvan's suggestion here from a more radical suggestion. Instead of assuming that the world has a structure or can have one imposed on it, one could also *identify* the world with a particular mathematical structure.

> [In the interpretation step] we interpret the mathematical consequences that were obtained in the derivation step in terms of the initial empirical set up. We call this step *interpretation*. To establish an interpretation, a mapping from the mathematical structure to the empirical set up is needed. This mapping need not be simply the inverse of the mapping used in the immersion step – although, in some instances, this may well be the case. (*ibid*: 353)

Bueno & Colyvan stress that the mappings between the mathematical structure and the empirical set-up can, in some cases, be the mappings set up in the partial structures framework (da Costa & French 2003; Bueno, French and Ladyman 2002; French and Ladyman 1998). As a result, in addition to standard set-theoretic mappings like isomorphism and homomorphism, the structural relations will, in some cases, be partial mappings like partial isomorphism. The details of the partial structures framework are set out in appendix B. This distinctive feature of the account plays a role in the accommodation of idealizations, which will be discussed in more detail in §6.6 and in fn. 178.

Here are two brief interpretative remarks. First, it is unclear whether or not the mathematics used to derive the consequences, in the interpretation step, are part of the content of a given representation. Pincock calls mathematics that is part of a representation *intrinsic* and mathematics appealed to when making use of a representation *extrinsic*. Second, that the account is presented in terms of a sequence of steps that are carried out obscures the fact that the immersion step can be repeated *prior* to the derivation step, such that the mathematical structure into which the empirical structure has been embedded is embedded into a further mathematical structure. Similarly, the working through of the three stages can, itself, be repeated (Bueno & Colyvan 2011: 354).[169]

6.4. Mathematical representation and scientific representation *simpliciter*

Setting out the core commitments of the mapping account and the inferential conception gets us a little closer to understanding the nature of mathematical representation: they are both ways of making more precise the common view that mathematical representation has

[169] Raz and Sauer make this point and offer what they refer to as the *dynamical* inferential account (Raz & Sauer 2015): but as far as I can see, this point is recognised by Bueno and Colyvan and can be incorporated by the inferential conception.

something to do with structural relations, but both also make clear that there is more to mathematical representation than there just being a structural mapping between some structure and some part of the world. A natural question, given the purposes of this thesis, is: how does mathematical representation relate to representation *simpliciter*? Is mathematical representation different *in kind* to other kinds of representation, or, rather, is mathematical representation distinctive in virtue of the vehicle being mathematical? This is, essentially, the question of whether or not anti-exceptionalism is true about mathematical representation.

Little has been explicitly said on this question. I will present a positive account of this relationship in §6.5, arguing for a kind of anti-exceptionalism. Before doing so, however, it is necessary to briefly discuss a rival understanding of how the accounts above relate to existing accounts of scientific representation.

6.4.1. Pero & Suárez's anti-exceptionalist understanding of mathematical representation

The notable exception to the general lacuna noted in the introduction is two recent papers (one written jointly with Francesca Pero) by Mauricio Suárez. Pero and Suárez understand Pincock as offering a structural account of representation of the kind that they take the Frigg-Suárez objections to undermine.[170] Recall that the Frigg-Suárez objections are objections to (structural) similarity accounts of representation that exploit the (purported!) fact that these accounts *identify* representation with (structural) similiarity. The *non-necessity argument* offers up, for example, cases of representation where (structural) similarity is missing, the *non-sufficiency argument* invokes cases of similarity without representation, and so on.[171] It seems clear that Pero & Suárez take accounts of *mathematical* representation that invoke structural similarity to fall victim to these objections.

Pero & Suárez explicitly contrast the mapping account with Contessa's account discussed in the previous chapter:

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170 Pincock's account is not the focus of these papers. However, given that Pero & Suárez 2016 and Suárez 2015 are two of the few discussions of the accounts of mathematical representation that explicitly relate them to accounts of scientific representation *simpliciter*, it is worthwhile discussing what is said. Also – whilst the comments by Pero and Suárez are anti-exceptionalist in that they think that the mapping account is a version of an existing kind of account of scientific explanation, they clearly do not think that it can be the *correct* way to accommodate mathematical representation.

171 See §5.5.1 for discussion of all of the Suárez-Frigg objections and §5.5.2 for discussion of how they can be responded to whilst retaining a key role for structural similarity.

> There is by now a long tradition of structural approaches to scientific representation, starting in van Fraassen and Suppes to the most sophisticated accounts by Bartels and Pincock. The tradition's critics (Contessa, Frigg, Giere, Suárez, van Fraassen) have invoked putative counterexamples to structural notions (Pero & Suárez 2016: 71)

Carving up the associated views in a similar way, in a discussion of what he refers to as 'substantive' accounts of representation (where this is understood as accounts of representation that "claim that representation is some substantive or objective property or relation" (Suárez 2015: 37)), Suárez groups Pincock's account in with the structural accounts discussed in the previous chapter, saying that "champions of substantive accounts include Pincock, who defends structural isomorphism [...] yet other attempts at substantive accounts include Bartels, who defends homomorphism, French and his disciples, who defend partial isomorphism" (*ibid*: 37).

Pero and Suárez's comments misunderstand Pincock's account in two ways.[172] First, it is false that Pincock's account holds that representation should be *identified with* one particular structural relation between vehicle and target. Although structural relations clearly play a crucial role in Pincock's account, the specification also sets up denotation relations between the vehicle and target: *these* relations between vehicle and target are user-fixed. The intentions of the user enter into the picture in setting up the denotation relations both between the vehicle and the target and between parts of the vehicle and parts of the target. The second misunderstanding of the mapping account present in Pero & Suàrez's comments is the claim that Pincock thinks that isomorphism is the structural relation involved in every case of mathematical representation. This is a misreading first because, as just discussed, there are multiple relations between vehicle and target involved in each case of mathematical representation (on Pincock's account) – only some of these are structural, the others are denotational. Second, even limiting to the structural relations involved, Pincock is explicit that he holds that the relationship between vehicle and structure involved in representation can include both other set-theoretically stated relations (like homomorphism) as well as relations that must be stated mathematically (Pincock 2012: 31). It should be clear, then, that

[172] Suárez and Pero & Suárez do not mention the inferential conception: but given that the mapping account is lumped in with structural accounts of scientific representation in virtue of the fact that it appeals to morphisms of various kinds, I assume that they hold that the same goes for the inferential conception given that it also gives a key role to such morphisms.

Pincock's account as set out in Pincock 2012 is not subject to the Suárez-Frigg objections for much the same reason as the account of epistemic representation endorsed in the previous chapter is not. It is no part of the view that structural relations between vehicle and target are either *identified with* or *sufficient for* representation. It is for this reason that the error made by Pero & Suárez, corrected in this section, is not an insignificant one. Rather, it is misleading because it suggests that Pincock's account (and Bueno & Colyvan's) is vulnerable to objections that it is in fact not vulnerable to.

Nevertheless, what is commendable about Pero & Suárez's remarks (however brief) is that they offer an answer what has (so far) largely been a neglected question: that of working out how the mapping account and the inferential conception relate to the accounts of representation *simpliciter*. As demonstrated above, however, it is a mistake to think that Pincock's account identifies the representation relation with a structural relation, or takes structural similarity to be sufficient for representation. Instead, representation also involves the setting up of denotation relations. Recall, also, that the discussion in the previous chapter demonstrated that there *is* a crucial place in the epistemic representation framework for structural relations. Taking the lead from this observation, from the fact that mathematical scientific representations seem like they are epistemic representations *par excellence,* and from Pero & Suárez's explicitly *contrasting* of Pincock's account with Contessa's, in the next section I argue for the opposite conclusion – that the mapping account and inferential conception can, in fact, be demonstrated to be *consistent with* the epistemic representation approach. Even though the two accounts give crucial roles to structural similarity, it is too quick to conclude that the accounts are guilty of *identifying* the representation relation with the structural relation (in the way that Suárez at least thinks leading accounts of scientific representation do). I will take the above grouping of the mapping account in with accounts that (purportedly) *reduce* representation to structural similarity as the foil of the next two sections. I first recall the crucial features of epistemic representation and then, in the following section, by arguing that the mapping account and inferential conception have these features. This both offers a corrective to Pero & Suárez's assumption, and allows for an anti-exceptionalist understanding of mathematical representation, shedding light on what mathematical and non-mathematical epistemic representations have in common.

6.4.2. The core features of epistemic representation

In this section I briefly recall the key features of the epistemic account endorsed in the previous chapter, in order to argue for the anti-exceptionalist conclusion in following sections.

The first relevant dimension of epistemic representation is that separate relations between the vehicle and target are responsible for the different functions of a (partially) faithful epistemic representation. I will refer to this as the *tripartite condition*. Being explicit about the relations between vehicle and target that ground the tripartite nature of faithful epistemic representation made plausible a view of representation that (a) allows that a minimal kind of representation (representation *qua* denotation) can be achieved near trivially (b) avoids the Frigg-Suárez objections whilst (c) allowing structural relations to nevertheless play a central role in scientific representation. This results in the second condition, which is as follows. A vehicle counting as a representation of a target (*qua* denoting its target) and a vehicle counting as an epistemic representation of a target are binary: a vehicle either denotes or it does not, and a target has either been interpreted in terms of a vehicle or it has not. Faithfulness, however, comes in degrees. Contessa refers to this aspect of epistemic representation as faithfulness being *gradable* (Contessa 2011: 129) so I will, accordingly, call this the *gradability condition*.

Much as with the accommodation of extra-mathematical explanation into Woodward's account of explanation, in this thesis I do not offer a full defence of Contessa's account of representation. Rather, making it plausible that Contessa's account is viable and focusing on the anti-exceptionalist accommodation of distinctively mathematical scientific representation is a productive strategy. I take its three features, (a), (b) and (c) above to (partially) ground its plausibility. How the anti-exceptionalist account of distinctively epistemic mathematical representation here fares against other potential anti-exceptionalist accounts (which would attempt to accommodate mathematical representation into a *different* pre-existing representational framework that can also be independently motivated) is a task to be carried out as and when such accounts emerge.

6.5. Mathematical representation as epistemic representation

In this section I argue that the mapping account and inferential conception can be understood as having the core features of the epistemic representation framework by demonstrating that they meet the tripartite condition and the gradability condition. This means that there is something substantive and (plausibly) fully general that can be said about all kinds of epistemic representation, ranging from maps to mathematical representations.

6.5.1. The tripartite condition

The tripartite condition says that different (sets of) relations should ground the fact that a vehicle denotes its target, the fact that a vehicle is an epistemic representation of its target and that a vehicle is a (partially) faithful epistemic representation of its target. The first motivation for the tripartite condition, remember, is the fact that representation *qua* denotation and epistemic representation are easy to achieve whilst faithful epistemic representation is not. The second motivation is the desire for an account that gives structural similarity an important role whilst not being susceptible to the Frigg-Suárez objections. If the mapping account and inferential conception appeal to structural relations in order to ground a representation's status as a representation *qua* denotation, then they seem left open to these objections. In this section, I will argue that they do not.

In what is perhaps the most succinct summary of his view concerning how a mathematical representation is set up, Pincock says the following:

> First, we must fix the abstract structure which we are calling the model. […] Then some parts of this abstract structure must be assigned physical properties or relations. At this second stage the parts of the purely mathematical entity are assigned denotation or reference relations. Finally, a structural relation must be given which indicates how the relevant parts map onto the target system or target systems of the representation. At the end of these three steps, the representation has obtained its representational content. (Pincock 2012: 257) (*cf. ibid:* 27)

Straightforwardly, then, for Pincock a representation obtains once these three steps are carried out and the three steps mirror Contessa's three (sets of) relations. The specification relations set up the two collections of denotation relations and the structural relation holds between the (structure of) the target and the vehicle once the denotation relations have been set up. Even though Pincock at one point contrasts his account with Contessa's (see §6.6.2 for discussion of this point), this three-step setting up of a representation clearly brings to mind, and even mirrors, Contessa's presentation of the three steps involved in setting up a (partially) faithful epistemic representation (Contessa 2007: 57) – the selection of a vehicle (and the setting up of a denotation relation between it and the target), the establishing of denotation relations between *parts* of the vehicle (for Pincock, the model) and *parts* of the target (via an interpretation) and then the holding of a structural relation. The above passage, I think, reinforces the argument made in §6.4 that (contra Suárez), the mapping account does not in any sense take the structural relation to be sufficient for representation – the first two steps are also crucial and without them, there is no representation (either *qua* denotation or epistemic representation). The mapping account does not, therefore, assign a *problematic* role to structural relations: they ground a representation's faithfulness (or, for Pincock, set the conditions of accuracy) rather than making it such that the representation denotes the target (which is what seemed to leave naïve views open to the Suárez-Frigg objections). If there were a structural mapping between a given model (to use Pincock's vocabulary) and a target system, without denotation relations having been set up, a representational relation of any kind would not hold, according to Pincock's account.

Things are not *quite* as straightforward for the inferential conception. On the inferential conception, the vehicle denotes the target in virtue of the intentions of the agent, in that the representational vehicle is selected by the user: the mathematical structure is a representation of *that* particular target system by stipulation, or something similarly user-fixed. However, recall also that Contessa requires that different relations are responsible for the fact that the vehicle is an *epistemic* representation and for the fact that the representation is *faithful* – for Contessa, the interpretation of the vehicle in terms of the target secures epistemic representation, while some kind of structural relation between the vehicle and target is responsible for the vehicle's degree of faithfulness. However, on the inferential conception, the relation that seems closest to being constitutive of the user's interpretation of the vehicle (to use Contessa's vocabulary) seems to be the structural relation invoked in the immersion step, when the user takes a given structural mapping to hold between the empirical set up and mathematical structure. This involves "relat[ing] the relevant aspects of the empirical situation with the appropriate mathematical context" (Bueno & Colyvan 2011: 353). Yet, on first glance, it seems as though this *very same* structural relation is responsible for the vehicle's faithfulness, if we are to follow Contessa's suggestion that structural relations are responsible for the faithfulness of a given epistemic representation. This generates worries that the inferential conception, unlike the mapping account, does not allow for different relations between (or facts about) the vehicle and target to be responsible for the different dimensions of epistemic representation.

It's first important to note that the utility of the tripartite nature of Contessa's account is grounded in its ability to yield a response to the Suárez-Frigg objections. All that's required in order for the response to be successful is that the account does not make the existence of a structural relation to be either necessary or sufficient for either representation *qua* denotation or epistemic representation. Secondly, and most importantly, there is a subtle difference between the two ways that a structural relation plays a role in setting up a (partially) faithful epistemic representation on the above understanding of the inferential conception. The interpretation of the vehicle in terms of the target occurs when the user of the representation *takes the structural relation to hold* between the physical and mathematical domains – and this taking is user-fixed. This is what happens on Bueno and Colyvan's immersion step, with "the relevant aspects of the empirical situation" being related to the "mathematical context" (*ibid*), via taking a mapping to hold between the two. It is at this stage that we should understand denotation relations as being set up between the vehicle and target. In contrast, a structural relation *actually obtaining* between the vehicle and target is what makes the representation faithful and the eventual inferences sound. On this line, the establishing of denotation relations between (parts of) the vehicle and (parts of) the target is something like a by-product or symptom of *taking there to be* structural similarity – see fn.173 for another way of articulating this idea. This seems right – whatever fixes the interpretation of the vehicle in terms of the target ought to be something user-fixed and *taking* the two domains to be structurally similar in the way stated in the immersion step seems like this sort of fact. Whatever makes a representation faithful, and the inferences sound, should be something non-user fixed, and worldly (as it were) – something about the vehicle and target, and not about the user. The vehicle and target *in fact* being structurally similar (in the way that the user takes them to be) is what plays *this* role.

One might worry at this stage that this bifurcation is *ad hoc*, and is motivated *only* by the anti-exceptionalist aim of making the inferential conception and Contessa's account harmonious. This worry can be assuaged by considering a non-mathematical example of epistemic representation. Suppose a city trekker versed in the vocabulary of set-theory takes a map to be a map *of* Berlin, and states that they take the map to be isomorphic to Berlin (or, speaking more carefully, to some structure that Berlin can be taken to instantiate). One might worry that on the *letter* of Contessa's account, a case of epistemic representation has not been established: the map is taken to denote the city and a structural relation is taken to obtain between Berlin and the map, but denotation relations have not been established between parts of the map and parts of Berlin. Yet, it seems obvious that in this case, parts of the map have been taken to denote parts of Berlin *when the user of the representation takes the two to be isomorphic!* Taking the two to be isomorphic involves establishing a function that takes as its input exactly one map-object (or property or relation) and maps this to exactly one city-object (or property or relation), but these mappings can *also* very easily be understood as denotation relations, or at least as *also yielding* denotation relations. In this case, an isomorphism between Berlin and the map are playing the dual role of acting as an interpretation *and* in grounding the faithfulness of the map.[173]

Here is a final worry along these lines. In the Berlin map case, it seemed very natural to think that in taking the map to be isomorphic to Berlin, denotation relations were being set up between objects (and properties and relations) in the map and objects (and properties and relations) in Berlin, however implicitly. One might worry that the same will not be true of all cases drawn from scientific practice in which a mapping is proposed between a physical and mathematical structure: that the account is guilty of a certain naivety, resulting from taking the analogies with epistemic representations like maps too seriously. One intuitively problematic case is permutation invariance. The basic idea here is that there are physical systems with particles such that these systems are invariant under permutations of these particles. This might be taken to threaten the idea that, in stating that a morphism of particular kind is taken to hold between the physical and mathematical structure, denotation relations are implicitly being set up between objects in the two domains: because, in this case,

[173] Again, being slightly more careful: the user *taking Berlin to be isomorphic to the map* is what grounds the interpretation of the map in terms of Berlin and Berlin *in fact being isomorphic* to the map is what makes the map faithful. So, even though a structural relation between vehicle and target is in some sense playing the dual role, the user having a belief with the isomorphism as its object is playing the role of fixing an interpretation, but the isomorphism *in fact obtaining* is playing the role of grounding faithfulness. The same kind of bifurcation should play a role in understanding the inferential conception *qua* account of mathematical epistemic representation: to immerse the empirical set up into the mathematics is to take the chosen structural relation (which will, in mathematical cases, *not* be isomorphism) to hold between the empirical set up and the mathematical structure is to interpret (in Contessa's vocabulary), but the inferences produced will only be sound if this chosen structural relation *actually does* hold between the empirical set up and the mathematical structure (and *this* is not user-fixed).

it is understood by the user of the representation that these objects in the target system are, for the current purposes, indistinguishable. There has been extensive philosophical discussion of this kind of symmetry, and of permutation invariance – most relevantly, but non-exhaustively, on whether or not permutation invariance reveals that quantum particles are, in fact, not individuals (what French & Rickles call 'the Received View' (French & Rickles 2003)) and on the consistency of permutation invariance and an ontology of particles (Jantzen 2010). It would be unsurprising, then, that this should be a case that might threaten the joint assumption that denotation relations are established between *parts* of the vehicle and target, in addition to between the vehicle and target. Having flagged up this potential open question here, I will return to it when discussing the limits of the anti-exceptionalist accounts offered in the conclusion.

6.5.2 The gradability condition

On Contessa's account of epistemic representation, faithfulness comes in degrees. Following Contessa, I am referring to this as gradability. Although the fact that faithfulness comes in degrees is a component of Contessa's particular account, I also take it to be a platitude about epistemic representation to be accommodated by any such account: two representations that denote the same target can be more or less faithful *epistemic* representations (where this will be connected (non-trivially!) to the representation's usefulness). Although in this context meeting the gradability condition is a worry about making the accounts of mathematical representation consistent with, or part of, the epistemic representation framework, it is an instance of a more general worry, already noted by both Suárez and Contessa. Contessa notes that taking structural relations between vehicle and target to ground a representation's faithfulness (as he suggests) generates the tension that faithfulness is a matter of degree whilst the holding of a structural relation is not (Contessa 2011: 129):

> A more serious problem is that the notion of faithful epistemic representation is a *gradable notion*, but that of morphism is not; whether or not that morphism holds between the structure instantiated by the vehicle and that instantiated by the target is a yes-or-no question but how faithful an epistemic representation of a certain system is of a certain target is a matter of degree (Contessa 2011: 129) (emphasis added)

Suárez's argument from inaccuracy, too, pointed out that structural relations holding is binary whilst accuracy is not (§5.5.1.) This is not, then, a *new* problem that arises from attempting to achieve the irenic aim of rendering the mapping account and inferential conception consistent with Contessa's approach. It is unsurprising that, with their natural focus on structural relations, this problem raises its head here. This means that there exist the resources to resolve the apparent tension.

First, though, note the following aspect of the mapping account. Pincock's approach focuses on accuracy conditions rather than inference, in a sense that is potentially problematic. On Pincock's account, mathematical scientific representations set accuracy conditions on the world (Pincock 2012: 28). Accuracy conditions are understood as restrictions on how the world must be if the representation is accurate. As should now be obvious from Contessa and Suárez's comments recalled above, this has the consequence that the accuracy or correctness of a representation is binary, rather than gradable – a representation is accurate if the world 'holds up its end of the bargain' (so to speak), and it is inaccurate if it does not. Pincock, therefore, seems to have a deviant notion of accuracy, one that does not admit of degrees. This suggests a worry that we will be unable to compare two representations of the same target.

There is, furthermore, something of a related tension in Pincock's account. Pincock says, as noted in the quotes above, that the content of a mathematical representation is entirely structural. Yet, it also seems clear from his comments that the existence of a structural relation between the (structure of) the vehicle and the (structure of) the target is insufficient for the former to represent the latter – denotation relations must also be set up via the specification. So, even though these denotation relations aren't part of (what Pincock calls) the *content* of a representation, they do nevertheless play a necessary role in bringing it about such that the vehicle represents the target. Both this tension and Pincock's idiosyncratic notion of accuracy can have light shed on them by appealing to the epistemic representation framework. Furthermore, although I have brought out this tension by appealing to the details of Pincock's account, the first worry transfers over to the inferential conception: a structural relation holding is binary, whilst faithfulness comes in degrees and the inferential conception appeals to the structural relations of the partial structures framework (see fn. 175).

Recall, however, that once structural mappings are assigned the role of fixing faithfulness rather than fixing whether or not the vehicle is a representation of the target *at all* (a suggestion made, in different ways, by Chakravartty and Contessa, and endorsed in §5.4.4), those who are sympathetic to structural accounts of scientific representation needn't try and identify a single morphism to constitute the structural relation between vehicle and target across all scientific representation (Contessa 2011: 129). Rather, different representations will make use of different structural relations (or, more carefully, different representations will be faithful in virtue of different structural relations). Some epistemic representations will make use of relations as strong as isomorphism to ground the faithfulness of the representation, but others may appeal to weaker relations. According to this natural thought, the degree of faithfulness of a representation is determined by how structurally similar the vehicle and the target are – and this, in turn, is fixed by (or, rather, is identical to) how strong the morphism is between the structure instantiated by the target and the vehicle structure are.

Contessa appeals to this idea in unpublished work (with permission given to cite). Building on the work in Contessa 2007 and Contessa 2011, he suggests that the faithfulness of a given epistemic representation is determined by the strength of the morphism between the vehicle and structure (Contessa ms: 87 – 94). Whilst this offers a useful way to address an apparent tension between the accounts of mathematical representation discussed here and the epistemic account, it nevertheless generates questions about Contessa's account. There are now two independent ways to measure the faithfulness of a given representation. The first is (something like) the ratio of sound to unsound inferences licensed by the representation: the greater the percentage of these inferences that are sound, the more faithful the representation. The second is (something like) the strength of the morphism between the structure of the target and of the vehicle determines the faithfulness of the representation: the stronger the morphism, the more faithful the representation.[174]

Nevertheless, it seems like a similar solution can be given in order to render faithfulness a matter of degree on the mapping account and inferential conception. Different representations can be compared with respect to their faithfulness by considering the structural relations that *ground* their respective faithfulness. As long as a good enough story can be told about the relationship between the number of sound inferences licensed by a vehicle and the strength of the morphism between the structure of the vehicle and the

[174] Given that Contessa has moved away from working on scientific representation, it is unclear as to whether or not a canonical answer will be given to this question, as it were. I suspect that by appealing to the fact that the relevant structure of the vehicle is one chosen by the user, these two ways to measure faithfulness can be rendered consistent: this is a topic for future work.

structure of the target, the faithfulness of an epistemic representation can be measured by the strength of the morphism that obtains between the two domains.[175]

6.6. Inferences, maps, means and ends

This irenic result that both the mapping account and the inferential conception can be accommodated by the epistemic representation freamework tells against the inference from the surface level focus on structural similarity to the thought that the accounts, therefore, *identify* representation with structural similarity. This is the inference that I took to be present in Pero and Suárez's remarks and their contrasting of the mapping account with the epistemic representation account. It is also an anti-exceptionalist outcome. However, it *does* sit slightly oddly with the view that the former two accounts are presumably supposed to be understood as *competing* accounts of mathematical representation. In this section I will accordingly suggest one way of dissolving the apparent differences between the two accounts.[176]

6.6.1. Purported advantages of the inferential conception over the mapping account

Bueno & Colyvan discuss difficulties with the mapping account that, they, claim, can be resolved by their proposed alternative (Bueno & Colyvan 2011: 356 – 359). The first is the purported fact that the mapping account does not "have the resources to accommodate the fact that mathematical theories often have more structure than the empirical set up, and some of the additional structure (suitably interpreted) has empirical implications" (*ibid*: 356). They suggest that the inferential conception has the ability to avoid this problem in virtue of the fact that the structure selected at the various steps of the process are "completely dependent on context" – that "the choice here [between various mathematical structures and various

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175 A suggestion much along Contessa's line above has already been suggested by da Costa and French in their discussion of the partial structures framework (which, recall, is appealed to by Bueno and Colyvan) – so all that is needed is to deploy it for the slightly different purpose of rendering the inferential conception harmonious with the gradability condition. Recall that, on the inferential conception, the structural relations between vehicle and target that ground both the vehicle's status as an epistemic representation and the representation's faithfulness will often be those found in the partial structures framework. The relations of the partial structures framework are commonly understood as an ordered triple, $R = <R_1, R_2, R_3>$, where R_1 is the set of *n*-tuples that hold for R, R_2 is the set of *n*-tuples that do not hold for R and R_3 is the set of *n*-tuples for which it is not known, or for which it is not defined, whether or not they belong to R (see appendix B for details). da Costa and French, in proposing the partial structures framework, suggest that it can be used to measure the degree of similarity or approximation involved in a model. I take it that, for Bueno and Colyvan, this is one of the advantages of appealing to the partial structures framework.

176 The suggestion here is not that Bueno and Colyvan misunderstand Pincock's view: the version of Pincock's view that they target in Bueno & Colyvan 2011 is less sophisticated than that which is set out schematically at the beginning of Pincock 2012 and then developed throughout.

structures of the target system] will emerge from a careful consideration of the benefits and costs of each option" (*ibid*). It is unclear, from their discussion, exactly how the fact that the particular morphism is selected by appealing to contextual factors is supposed to accommodate this worry: but, in as much as it does, in his later presentation Pincock *also* stresses that the relevant structural relation between vehicle and target taken to play a role in a given representation will be informed by contextual factors (Pincock 2012: 31-33).

The second concerns the worry about assumed structure of the target system, discussed above: the need to "impose some structure on the world in order to begin the modelling exercise" (Bueno & Colyvan 2011: 357). However, it seems clear that Pincock's specification step should be understood as exactly imposing some structure on the world – given that it involves dividing the target system into the salient objects, properties and relations in the target such that they can stand in denotational relationships with mathematical objects, properties and relations. There are, of course, deep questions about whether or not any one of the potential structures that the physical world is taken to instantiate is (in some sense) a privileged structure and whether or not an account of mathematical representation along these lines is committed to holding that the answer to this question is yes *and* that this privileged structure is latched on to.[177] But, for the purposes of comparing whether or not the inferential conception has any resources *over and above* the mapping account in answering this question, these needn't be settled here.

The third concerns accommodating idealizations. Bueno and Colyvan argue that, because the inferential conception appeals to the partial structures framework (see appendix B), idealizations can be accommodated in a way that the mapping account cannot. There are two ways of dissolving this potential difference between the accounts. First, there appears to be nothing in the details of the accounts that prevents the mapping account from *also* appealing to the partial structures framework (although see fn.178). Second, in the updated presentation of the mapping account in Pincock 2012, Pincock devotes much discussion to how his account can accommodate idealizations, or falsehoods incorporated into a representation. Here's a quick example to demonstrate the basic strategy, following Pincock's discussion (Pincock 2012: 29-31). Pincock distinguishes between basic contents ("simple

177 For reasons that are familiar from the debate about forms of structural realism, this is not an easy question to address. As Newman demonstrated (Newman 1928), any target system is isomorphic to any structure that has the same cardinality as the target and so could be taken to instantiate that structure. See Ketland 2004 and Ainsworth 2009 for discussion.

kinds of structural relations that are defined in terms of the basic elements of the concrete and mathematical systems" (*ibid*: 29)) and schematic contents, which involve "derived elements […] used to represent physical entities beyond those that can be related directly to the mathematical entities that appear in the domain of the mathematical structure" (*ibid*). He introduces the example of the heat equation being used to represent an iron bar changing temperature:

(4) $$\alpha^2 u_{xx} = u_t$$

Pincock first suggests that his account might say that "we should say that this representation is accurate when we have an isomorphism between the temperature at each point at each time and the set of ordered pairs (x, t) picked out by the solution" of the equation (*ibid*). Given the oddness of this, Pincock then suggests that, rather, if it is permitted to use mathematics to specify the structural relation, instead "we may posit an isomorphism between the temperatures at times and *u* in the mathematical structure subject to a spatial error term $e = 1$mm" (*ibid*). Pincock then goes on to give a description as to how his account accommodates contexts in which the iron bar is represented as equally long (see *ibid*: 31-33 for details).

In short, idealization is accommodated by allowing the structural relations involved to be "more complicated than just simple isomorphisms and homomorphisms […] we allow the specification of the structural relation to include mathematical terminology" (*ibid*: 31). (See, also, his later remark that "the content of [mathematical representations] is analysed in terms of the existence of a structural relation whose features depend on the mathematical structure involved and in some cases additional aspects of the relation itself that must be specified in mathematical terms" (*ibid*: 197)). The fact that Bueno & Colyvan's discussion locates a lacuna in Pincock's account where there, perhaps, is not one can be explained by the fact that they are responding to an earlier presentation of the account, where Pincock indeed describes the structural relations in a way that suggests that they are limited to the standard set-theoretic mappings:

> Counting, for example, involves isomorphisms from the objects counted to an initial segment of the natural numbers. More sophisticated applications will involve other kinds of

> mappings, such as homomorphisms that respect certain features of the physical situation, e.g., the mass of physical objects. (Pincock 2004b: 69)

What is interesting is that even though the crucial role played by inference is what most obviously distinguishes Bueno & Colyvan's account from Pincock's, its heavier emphasis on the inferential capacity of mathematics seemingly plays no role in articulating the supposed advantages of their account. Accordingly, the more sophisticated account set out in Pincock 2012 can appeal to much the same resources as the inferential conception does in addressing these worries (such as allowing for a role to be played by contextual factors, and allowing the relevant structural relations to be more sophisticated that isomorphism and homomorphism (although, again, see fn.178)).

6.6.2. Mathematical representation and means and ends

It is clear that Bueno and Colyvan think that the inferential conception is superior to the mapping account, but this is not clearly borne out by the details of the most recent version of the latter account. What, then, does Pincock think the relationship is between the inferential conception and the account set out in the first half of Pincock 2012? Pincock does not offer a direct comparison at any point between the mapping account and the inferential conception (beyond noting that working out how the inferential conception relates to the mapping account is a "delicate matter" (Pincock 2011: 211). Instead, he seems to group the inferential conception with a wider collection of views about representation and contrasts the mapping account with these. Given that he says reasonably little, it is worthwhile quoting it in full:

> Perhaps the main competitor to an approach based on accuracy conditions tends to put inferential connections at the heart of their picture of representation [listing Bueno & Colyvan 2011, along with Contessa 2007 and Suárez 2010b in a footnote]. Inferential approaches must explain the scientific practice of evaluating representations in terms of their accuracy. Although there does not seem to be any barrier to doing this, I have found it more convenient to start with the accuracy conditions. On my approach, inferential claims about a given representation follow immediately from its accuracy conditions: a valid inference is accuracy-preserving. (Pincock 2012: 28)

In this passage, Pincock construes inferential approaches as competitors to his account, yet spells out his motivations for starting with accuracy conditions as those involving convenience, rather than in virtue of such an approach yielding explicitly stated benefits. Something about this state of affairs should remind us of a tension discussed in the previous chapter. One way of approaching the relationship between the mapping account and the inferential conception, and their differing emphasis on inference and accuracy conditions, is to return to Chakravartty's distinction between functional and informational accounts of scientific representation, discussed in §5.4.4.

Recall that Chakravartty distinguished between informational and functional accounts of scientific representation (Chakravartty 2010: 198): the former set out relationships between vehicle and target that make it such that the vehicle represents the target whilst the latter explicate the difference between representations and non-representations via their functions. In his discussion, Chakravartty suggests that the supposed disagreement between the two may be illusory – informational accounts are involved in detailing the means of representation, whilst functional accounts explicate their ends (*ibid*: 199).

Given what I have said in this chapter about the relationship between scientific representation *simpliciter* and mathematical scientific representation, it should not be surprising that this distinction needs to be drawn again in this context. As might be taken from Pincock's comments above, perhaps we ought to reconfigure the mapping account and the inferential conception in the same way that I suggested in the previous chapter (leading on from Chakravartty and Contessa's suggestions): informational accounts of representation should be repurposed as accounts of accuracy or faithfulness of a representation, rather than offering the relations between vehicle and target as being constitutive of representation. *Something like* this move can be made here, too.

We should understand Pincock's setting out of the content of a mathematical representation as consistent with what Bueno and Colyvan say in their discussion. The structural relations that Pincock invokes in his account should be understood as those involved in grounding the faithfulness of a given representation, rather than being involved in *making it such that* the vehicle represents the target. Pincock says that the holding of the structural relation between vehicle and target is what makes a representation accurate (Pincock 2012: 28) and this seems akin to the structural relations grounding the faithfulness of a representation. In the vocabulary of Contessa's epistemic account, then, Pincock's view as stated is a view of *faithful* epistemic representation: that the vehicle is an epistemic representation *simpliciter* of the target is secured by the two types of denotation relation involved in what Pincock calls the specification step – the first two steps that, for Pincock, are involved in setting up a representation.

On the letter of Pincock's account, a representation is accurate if a relevant structural relation holds between the (structure of) the vehicle and the (structure of) the target and an accurate representation licenses sound inferences. On the letter of Bueno & Colyvan's account, a representation licenses sounded inferences if the relevant structural relation holds between (the structure of) the vehicle and the (structure of) the target, and a representation is accurate to the extent that it licenses sound inferences. The distinction is one of priority and emphasis rather than one of substance: the common core of both accounts states that a representation is accurate, and licenses sound inferences, if the relevant structural relation holds between the (structure of) the vehicle and the (structure of) the target.

With the incorporation of any *particular* informational account of representation into a particular functional account (understanding the former as offering information about the faithfulness of a representation, as in §5.4.4), there are likely to be remaining problems, both interpretative and substantive. The same goes here. I'll flag up one outstanding disagreement, concerning the nature of the structural relationships between the physical and mathematical domains. Bueno & Colyvan take it to be the case that these relations will be complete or partial morphisms whilst Pincock is explicit that he thinks that the structural relations between vehicle and target will, in some cases, be such that mathematics is required to articulate them, as in his discussion of the error term in the heat equation case study. Interestingly, both Bueno & Colyvan's move to the tools of the partial structures framework and Pincock's move from relations that can be stated set-theoretically (or, indeed, using only second-order logic) to relations that are standardly constructed using mathematics are made for the same reason: to accommodate idealizations into their respective accounts, in order to accommodate the fact that there will be cases in which a vehicle's usefulness can come apart from its faithfulness. Even granting that both accounts can be accommodated into the epistemic representation framework, this seems to nevertheless be a point of disagreement. How our accounts of scientific representation should accommodate idealizations is the subject matter of a different thesis and I cannot discuss this further here: it requires, also, a full account of the relationship between the usefulness and the faithfulness of a representation. I think, however, this bracketing is safe enough. This should be understood as an internal question about the resulting account of mathematical scientific representation, one about what relations the physical and mathematical domain stand in. Whether or not the relations that the (structure of) the target system and the (structure of) the physical system stand in must sometimes include those that must be stated using mathematics does not clearly have ramifications for the fact that both accounts agree that what grounds a mathematical vehicle epistemically representing a target is a collection of denotation relations and what grounds a mathematical vehicle *partially faithfully* epistemically representing a target is the existence of some structural mapping between the (structure of) the vehicle and the (structure of) the target. Having flagged this up as an area of future work, I will return to it in the thesis conclusion.[178]

6.7. Mathematical epistemic representation

In the previous section I suggested that, whilst there are remaining interpretative questions, there is something of a common core between the mapping account and the inferential

[178] Here's another outstanding question that would take us too far afield. Those working in the partial structures tradition generally take themselves to be describing scientific practice at (what French calls) the meta-level, where this contrasts with the object-level: whilst scientists use representational devices at the object level to represent the world, these philosophers of science take themselves to be involved in representing *those representational devices used by scientists* (da Costa & French 2003). To represent the representational devices used by scientists *as* set-theoretic structures is supposed to be non-committal to whether or not such devices *are in fact* set-theoretic structures. Those that favour this distinction hold that their view is consistent with the fact that, for example, "in the context of the quantum revolution, it was group theory, not (partial) set-structures that were effectively doing the (physical, mathematical and hence object level representational) work" (French 2012: 21). Rather than directly describing the representational relations going on at the object-level, what is offered is (something like a) *rational reconstruction* of scientific practice at the object-level. Curiously, the object/meta-level distinction does not explicitly raise its head in the mathematical representation literature. Even though Bueno and Colyvan directly appeal to partial structures, in their presentation it sometimes seems as if that partial structures are being offered as the structural relations that are being used *at the object level*, holding between representational devices and the world (taken to be appropriately structured) rather than at French's meta-level. This is obviously a departure from how those working in the partial structures tradition understand the framework. A reader of Bueno & Colyvan 2011 (and Bueno 2016) who was ignorant of the background commitments of partial structures framework would be forgiven for taking partial structures to be the structural relation between the target and the vehicle (rather than as a representational device used to *represent* the relation between target and vehicle, a la French). Different aspects of Bueno & Colyvan's presentation pull in different directions: they say that "although there are no *full* mappings between the empirical world and the mathematical structures, there are *partial* mappings between these empirical and mathematical structures" (Bueno & Colyvan 2011: 358) (emphasis in original) but also that "we can *represent* formally the partiality of that information and the structural relations between the various components involved in terms of the notions of partial structure and partial relation" (*ibid*) (emphasis added). Assuming that Bueno & Colyvan *are* working at French's meta-level, whether this reveals a deep tension between Pincock and Bueno & Colyvan's account, or a way to dissolve the apparent disagreement between the two accounts regarding which structural relations are involved in mathematical representation, is a question for future work that discusses the partial structures framework in more detail (for example, considering whether or not the meta-level perspective is required at all. (See Contessa 2006; Landry 2007 and response in French 2012)).

conception. Importantly, and most relevantly, there is a common core regarding what *makes it the case that* a piece of mathematics is a partially faithful epistemic representation with a non-mathematical target. Tracing back the route from chapter 1, this is what is of relevance for shedding light on the debate regarding mathematical ontology. I admit that some of the ways that the representationalist nominalists characterise mathematics' role makes it sound like they hold that mathematics merely *denotes*: this is true, perhaps, of Daly and Langford's idea of indexing. Even if such commentators deny that mathematics is ever explanatory, I assume they will admit that mathematics does more than merely denote: it is the *epistemic* nature of mathematical representations that is most of interest. Accordingly, in this section I will set out (what I take to be) the common core, what I will call the account of distinctively mathematical epistemic representation. It will be largely familiar from the discussion of Contessa's account of epistemic representation in the previous chapter.

For a given mathematical epistemic scientific representation, the representational vehicle is taken to be a mathematical structure and the representational target is a non-mathematical target system.[179] The representational vehicle denotes the representational target in virtue of some collection of facts about the user(s) of the representation, which may or may not in particular instances be exhausted by an act of stipulation. Parts of the mathematical representational vehicle are taken to denote parts of the non-mathematical target system and in virtue of this interpretation inferences are licensed about the target system and, therefore, the vehicle is an epistemic representation of the target. A (partially) faithful epistemic representation licenses at least one sound inference about the target and the faithfulness of a given representation is grounded by structural similarity between the vehicle and the target.

6.7.1. Worked example: the Lotka-Volterra equations

In this section I work through an example of mathematical representation, making clear that light can be shed on it using the account of distinctively mathematical epistemic representation set out above.

Consider, once again, the Lotka-Volterra equations. First, consider the denotational relationships. There are two kinds of denotation relation involved in a given mathematical epistemic representation. The first is between the vehicle and target and the second is

[179] I will not discuss the possibility that mathematics could be used to represent other mathematical facts.

between parts of the vehicle and parts of the target. At the beginning of chapter 5, I gave a rough and ready characterisation, but more detail can be gone into here. What is denoted in this case? In the equations' original context, Volterra was aiming to account for some particular facts about fluctuations of the Adriatic fish market that had been previously noted by marine biologist Umberto D'Ancona.[180] Yet, it would seem a mistake to think that, whenever they are being used, what is being denoted is the Adriatic Sea prey-predator population systems: it is a platitude about the equations that they have been used to understand very many predator-prey populations, as well as predator-prey populations in general. The same mathematical structure, picked out by the same coupled equations, can denote different targets.[181] Whatever story is ultimately told about how 'the fish predator-prey systems in the Adriatic Sea' and 'predator-prey system' come to denote their respective targets will be told about how the denotational relationship is set up in the case of the Lotka-Volterra equations. Given the crucial role assigned to user-input at the denotation stage, the fact that the same equations can be used to represent very many targets is easily accommodated: just as a salt shaker can be made to denote Madagascar at one moment (*cf.* Callender & Cohen: 73), a new act of stipulation can make it such that it denotes the Indian Ocean.[182]

Next, consider the denotational relations set up between parts of the vehicle and parts of the target. The details can be gleaned from Volterra's discussion (Volterra 1923). Mathematical properties are taken to denote physical properties: t denotes time, P denotes the size of the predator population, V denotes the size of the prey population, β denotes the capture rate of the prey, γ denotes the prey/predator birth conversion rate, α denotes the growth rate of the prey and δ and death rate of the predators. With this second set of denotation relations

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180 For a full account of this historical episode, see Kingsland 1995. For Volterra's discussion see Volterra 1926. For recent philosophical discussion of the equations, see Weisberg 2007 and Weisberg 2013. Whilst the explicit incorporation of the equations into an account of distinctively mathematical epistemic representation here is my own, I draw on these discussions of the details of the equations.

181 There is an interesting question, here, about exactly what the vehicle is in these cases. Pincock sometimes writes as if it is the *equations* (for example) that are the vehicle, rather than the phase space characterized by the equations: yet, on the face of it, this can't be right. To do justice to this question here would require discussing fraught issues about the relationship between equations and the mathematical structures that they are taken to characterize.

182 The account endorsed here requires only the weak claim that, whatever facts are involved in establishing denotational relationships, the intentions of a representation user will be amongst them, even if in some cases they are not sufficient.

established, once a set of inference rules are accepted, the equations are a representation *qua* epistemic representation of the predator-prey population (or *a* predator-prey population).[183]

Finally, consider the fact that the equations were a (partially) faithful epistemic representation of the Adriatic sea predator-prey populations. That is, the equations were useful in Volterra's goals because they licensed inference to facts about the relevant population. In virtue of being (partially) faithful epistemic representations of other targets, the equations can be used to infer to facts about other such target systems. For example, as Weisberg notes, the representations can be used to demonstrate the Volterra principle (Roughgarden 1979: 439): "the population of prey will increase relative to the number of predators upon application of a pesticide" (Weisberg 2006: 735).

This, of course, is all very familiar. Yet, it is worth recalling what the above accommodation tells us. The above accommodation of the Lotka-Volterra equations is consistent with what Pincock says about the case (Pincock 2012: 58-60). The steps involved in setting up a mathematical representation, on the mapping account, have been taken. Yet, this is shown to be consistent, rather than opposed to, an account of representation that places inference at its heart. Working through this example, then, further reinforces the suggestion made above that both the mapping account and the inferential conception are in agreement, at least about what *makes it the case* that the equations represent the target, both *qua* denotation and *qua* epistemic representation.

A natural question concerns how mathematical and non-mathematical epistemic representation differ. Given that the view endorsed above is anti-exceptionalist, it is unsurprising that the differences between mathematical and non-mathematical representation end up being more slight than might be thought at first, given the surface level dissimilarity between a mathematical representation and a map (for example). One interesting difference, however, involves the knowledge that the user has of the structural relation between vehicle and target that is responsible for the faithfulness of the representation. In the case of many non-mathematical epistemic representations, the user may be ignorant of what relationship between vehicle and target is responsible for the fact that many of the inferences they draw are sound inferences. If it is an isomorphism that

[183] Although we are likely to find the term 'denote' far less commonly in the scientific literature (it already being, in this context, a slightly extended use of a philosophers' term of art), listing mathematical terms alongside the aspects of the target system they represent, setting out an interpretation or stipulation, is very common.

grounds the faithfulness of the subway map, this need not be known by the user of the representation: one does not need a lesson in set theory in order to navigate their way around Glasgow, nor would it help.[184] In contrast, in the mathematical scientific cases, a user of a given representation may be more conscious of the potential structural relations between the vehicle and the target. As in Bueno & Colyvan's presentation, it is often the case that specifying a structural relation between vehicle and target will 'double up' as setting up the denotational relationships between (parts of) the vehicle and (parts of) the structure. This need not be a threat to the account, however: it is likely to be a consequence of the fact that the relevant mathematical and physical structures will be far more complex than the structure of the map and subway system, such that using heuristics in order to construct an epistemic representation is far less likely to be successful.

6.7.3. Recap

Before returning to the metaphysical considerations that first led to the discussion of mathematical representation in this chapter, it is worth briefly making explicit once more the relationships between representations of different kinds. Recall six of the representations discussed in the preceding two chapters: the word 'cat', the hydraulic machine, the Glasgow subway map, an altered Glasgow subway map, the suspension bridge equations and the Lotka-Volterra equations.

The first way to collect these representations is to form the collection of every representation *simpliciter*, where to represent in this sense is (more or less) just to denote. This collection of representations includes 'cat', the hydraulic machine, the Glasgow subway map, the altered Glasgow subway map, the suspension bridge equations and the Lotka-Volterra equations. Second, the epistemic representations can be grouped together: this collection of representations includes the hydraulic machine, the Glasgow subway map, the altered Glasgow subway map, the suspension bridge equations and the Lotka-Volterra equations. A third, slightly smaller, collection can be formed by grouping together the (at least) partially faithful epistemic representations: this collection includes the hydraulic machine, the Glasgow subway map, the suspension bridge equations and the Lotka-Volterra equations. A fourth, slightly narrower still, collection is the collection of (at least) partially faithful scientific

[184] Nevertheless, if pressed to explain *why* a given subway map is a useful representation (in virtue of being (at least partially) faithful), a user is likely to offer up the suggestion that it is because it is similar to the subway system.

epistemic representations: this collection includes the hydraulic machine and the Lotka-Volterra equations. The final, smallest, collection of representations are the (at least) partially faithful distinctively mathematical scientific epistemic representations: this collection includes the Lotka-Volterra equations. To borrow from what is now a familiar phrase: there is no *special* problem of mathematical representation.[185]

6.8. The metaphysical and epistemic commitments of mathematical epistemic representation

The aim of this section of the thesis has been to get at the core commitments of a view about distinctively mathematical scientific representation. I argued that this was required in order to shed light on representationalist nominalism: the combination of Representationalism (the view that mathematics merely plays a representational role in science) and the Representation Conditional (the view, roughly, that if Representationalism is true than our world-oriented uses of mathematics is not metaphysically committing). In this section I draw on comments from Bueno & Colyvan and Pincock in order to discuss this question, in light of the account of mathematical epistemic representation endorsed above.

6.8.1. Bueno and Colyvan's metaphysical neutrality

Bueno and Colyvan suggest that the inferential conception is consistent with both realism and nominalism. They first note that:

> For the platonist, inferences used in the successful unification of different (mathematical) theories, or in the prediction of novel phenomena via the (indispensable) use of mathematical theories, or in the mathematical explanation of phenomena support the realist commitment to the corresponding mathematical entities. […] In every step of the application process, from the immersion through the derivation to the interpretation stages,

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185 Here's the small print (literally and figuratively). To say that there is no special problem of mathematical representation is to say that there is no special problem about *in virtue of what* a mathematical vehicle is a (partially) faithful epistemic representation of a non-mathematical target. There are parallels, then, with Callender and Cohen's claim about there being no special problems about in virtue of what a scientific representation represents, *qua* denotation, its target. There will, of course, be remaining questions about mathematical representation: including, but not limited to, questions about what structural relations ground faithfulness in this or that representation (and how these relations are determined), what it means to say that the target system *has* a particular structure that allows it to enter into structural relations with the mathematical vehicle, and what the relationship is between faithfulness and usefulness.

> mathematical entities are invoked, and thus, the platonist insists, we are ontologically committed to these entities. (Bueno & Colyvan 2011: 367)

They also suggest that the account could be endorsed by the nominalist. They offer two suggestions along these lines. The first simply suggests that the nominalist endorse Azzouni's distinction between quantifier and ontological commitment. The second is perhaps more interesting:

> [T]he nominalist may argue that, in applied mathematics, what is crucial is to make sense of the mathematical formalism in a physically significant way. For the nominalist, according to the inferential conception, both the immersion and the interpretation steps in the application process presuppose a particular physical interpretation of the mathematical formalism. […] Moreover, the nominalist can accommodate the derivation step without commitment to the existence of mathematical entities, by cashing out the notion of consequence in modal terms. (*ibid*: 367)

It seems, then, that on Bueno & Colyvan's quote, mathematics' representational capacity does not generate any *unique* or *special* motivation for realism: the real action depends on, for example, whether or not the distinction between ontological and quantifier commitment can be made to work, and if the nominalist can make do with a modal notion of consequence (Field 1989).

It's worth briefly noting Bueno and Colyvan's differing views. As is familiar, Colyvan is a Platonist whilst Bueno has made many contributions defending moves made in various debates by the nominalist (Bueno 2009; Bueno 2012). Given what Colyvan says in Bueno & Colyvan 2011 and elsewhere, it is clear that his Platonism is motivated by the explanatory considerations discussed in the first part of this thesis and *not* on the representational considerations discussed here.[186] In a more recent discussion, Bueno briefly alludes to the discussion in Bueno & Colyvan 2011 in order to make the case that there is an equally plausible anti-realist understanding of mathematics' representational capacity (Bueno 2016). Much as in the earlier discussion, however, his reasons for thinking that mathematics' representational role is not ontologically committing do not flow from any of the details of the inferential conception. Instead, they are grounded by the claim that morphisms of various

[186] Colyvan has been a noted critic of the kind of distinction that Azzouni tries to draw between ontological and quantifier commitment (Colyvan 2005; Colyvan 2010)

kinds can be construed in second-order logic, combined with an endorsement of the kind of teasing apart of ontological and quantifier commitment discussed by Azzouni (*ibid*: 2602).[187] Here are two worries about this strategy. First, as noted above, an outstanding question is whether or not there are cases in which the structural relationship between the mathematical and physical domain will be those that require mathematics to formulate (Pincock 2012: 27). This was recast as a debate *about* what features should be had by the account of mathematical epistemic representation rather than a debate *between* competing accounts, but it is a substantive debate nonetheless. If Pincock is correct, then (at the very least) it becomes an open question whether or not these can be reconstructed in second-order logic in the same way that isomorphism, homomorphism and the partial analogues can. Second, the prospects of this account rise and fall with the plausibility of cleaving the two kinds of commitment apart, but in Bueno 2016 there is little discussion of the various objections to both this distinction and the independent grounds given for existence. It seems that these questions must be resolved before this *particular* route to a nominalist understanding of mathematics' representational capacity can be fully defended.

6.8.2. Pincock's understanding argument

Pincock, contra Bueno & Colyvan, holds that mathematical scientific representation is not philosophically innocent, as it were: he claims that the fact that mathematical scientific representations have the kind of content that they do, and the fact that we can understand these contents, entails (or, more accurately, presupposes) a view about mathematics. In particular, Pincock holds that our representational practices presuppose truth-value realism. Crucially, his reasons for thinking this do not go via an argument against, for example, the cleaving apart of ontological and quantifier commitment. Although this argument has been mentioned in passing in commentary on Pincock 2012 (Walsh, Knox & Caulton 2014: 466) it has not been scrutinised yet in the literature. I do so here.

Pincock's central claim is the following:

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[187] It's not clear from Bueno's presentation of these two options whether or not he thinks that *one* of these routes being viable is enough to secure a nominalist understanding, or whether he thinks both are. I can clarify this point here. It strikes me that if the distinction can sensibly be drawn between quantifier and ontological commitment, then it does not matter if the relations between vehicle and target can be given a construal in second-order logic. The same cannot be said for the situation in which the relations between vehicle and target can all be construed in second-order logic but the distinction between quantifier and ontological commitment is not viable, however: in such a situation, our representations still seem to involve mathematical structures standing in structural relations (construed in second-order logic!) with the (structure of) the target system.

> **Understanding thesis:** in order to understand a mathematical representation, an agent must believe that the claims describing the mathematical structure are true.

The passage in which Pincock introduces the thesis (and as far as I can tell, presents his reasons for thinking that it is true) is below:

> The content [of a mathematical scientific representation] involved the existence of a structural relation of this or that sort between a specified mathematical structure and the target system. So, for an agent to *understand* this sort of representation, he or she must *believe* that the claims describing this structure are true. The process of prediction and testing operates only on those representations that a scientist can understand. (Pincock 2012: 217)

It seems right that a representation can only be used for the purposes of prediction if the representation is understood. However, it is not at all clear what reasons Pincock has for thinking that in order for a representation to be understood, it is necessary to believe the relevant claims about the mathematical structure involved. Pincock says very little about what notion of understanding he has in mind, which is inconvenient given that it is hardly a univocal term. It's also hard to pin down the exact form that the argument takes. It seems that it should not be understood as taking as its premises certain claims about our representational uses of mathematics and its conclusion truth-value realism.[188] Rather, the conclusion of the argument is (something like) the claim that everyone who understands a mathematical representation *is* a truth-value realist, because believing in the truth of the mathematical components of a representation is a pre-condition of this understanding.[189]

6.8.2.1. Epistemic understanding and the understanding thesis

[188] Pincock's argument does not, therefore, precisely mirror the enhanced indispensability argument in arguing from the fact that mathematics plays a particular kind of role in our scientific theories to some form of realism.

[189] This has interesting parallels with the debate about whether or not non-realists of various kinds can have the attitudes of acceptance towards theories, rather than belief. Horwich has argued that those advising *acceptance* of theories of some kind, in describing what acceptance amounts to, end up merely describing belief – and, as such, such epistemic anti-realists *do*, in fact, believe those theories that they claim only to accept (Horwich 1991). (See Leng 2010: 210-216 for discussion). Similarly, then, Pincock's claim must be that those who understand a mathematical representation but claim not to believe its mathematical content (either explicitly espousing non-belief or remaining agnostic) are wrong, much in the same way that Horwich thinks that epistemic anti-realists are wrong about their own attitude towards scientific theories.

One natural thought is that Pincock is appealing (however implicitly) to views in epistemology where understanding is a form of epistemic success.[190] It is common to think that many kinds of epistemic understanding are factive, suggesting this might be a good place to look to charitably reconstruct Pincock's argument.

Kvanvig has introduced a useful distinction between three different kinds of understanding. The first can be referred to as propositional understanding: understanding *that* something is the case (Kvanvig 2003: 191). The second can be referred to as explanatory understanding: where understanding *why* something is the case is associated with the possession of an explanation of the object of understanding (*ibid:* 200). The third is referred to as objectual understanding: "when understanding grammatically is followed by an object/subject matter, as in understanding the presidency, or the president, or politics" (*ibid*: 191). Discussions of understanding in the epistemology literature often draw on this distinction in order to make more specific claims about the nature of this or that kind of understanding (for example, debates about the factivity of understanding-why (Pritchard 2008; Baker 2003b) and objectual understanding (Zagzebski 2001)).

Propositional understanding is standardly taken to be factive: to understand *that* something is the case just is to know it (Kvanvig 2003: 191).[191] However, a dilemma emerges if Pincock pursues this unpacking. On the one hand, a representation doesn't seem propositional in nature. On the account above (which I suggested was consistent with what Pincock says), a (partially faithful) epistemic representation is obtained if the two forms of denotational relationship hold between the vehicle/parts of the vehicle and the target/parts of the target, and if there is structural similarity between vehicle and target. None of this is straightforwardly propositional. A representation does not take the form of a proposition *stating that* such and such denotational and structural relations obtain – a representation is *constituted by* these representations holding between vehicle and target.[192] If anything is a good

190 There are obviously thoroughly non-epistemic uses of 'understanding', such as Elgin's examples: "I can say 'I understand' to moderate the force of an assertion or hedge my claim. 'I understand that you are angry with me' may be a mild overture that gives you space to politely demur. This is a moderating use." (Elgin 2009: 322).

191 It is worth noting that commentators like Catherine Elgin take umbrage with the received wisdom that understanding (of various kinds) is connected to truth (Elgin 2004): given that I don't think that any of these standard epistemic understanding-truth links can help bolster Pincock's argument, I do not discuss this issue any further.

192 Remember that, even though Suárez takes aim at views that (he says) reduce representation to this or that relation, the Suárez-Frigg objections more narrowly had ramifications for views of representation that reduce representation to (structural) similarity relations. On the account here, some of the relations involved are denotational relations, established (in part) by the user of the representation.

candidate for being the referent of the term 'the representation', it is the representational vehicle, which is (in almost all cases of representation, at least) not a proposition. If pushed to understand a representation as propositional, the two most natural ways to do so is to take it to either be the proposition *that* the representation is successful, or to be the set of propositions about the vehicle that are licensed by the representation.[193] The first will not do, as I take it that Pincock thinks that we are perfectly capable of understanding unfaithful or inaccurate representations. I return to the second option below. The same kind of reasons carry over to explanatory understanding. Understanding-*why* must also, it seems, take a proposition as its object. It similarly will not do to think that the claim that in order to understand *why* a representation is a representation at all, it is necessary to believe the mathematical content. What grounds both of these is the fact that "if the primary unit of understanding is the proposition, then the difference between knowledge and understanding seems slight" (Elgin 2009: 333). Nor can the argument can be fleshed out appealing to an objectual reading of 'understanding'. It is even more difficult to make sense of a given representation as an area, or subject matter, in the same way that politics is (given that one of the most natural ways to explicate understanding of a subject area is to take this to be understanding-that of a collection of propositions that make up the subject area).

Initially, appealing to epistemic understanding seemed a promising way of making Pincock's understanding claim plausible: but, on closer inspection, it seems that no attempt along these lines is workable. This is symptomatic of at least two things: the first, as signalled above, that it is unclear whether or not representation is propositional and at least two of the kinds of understanding discussed by Kvanvig seem like they must take a proposition as their object. Second, it is symptomatic of the fact that, for Pincock, the link between understanding and belief does not extend to the entirety of the mathematical representation. Although each mathematical representation expresses some facts about the non-mathematical system (or licenses inference to these purported facts), Pincock does not think it necessary to believe *these* in order to understand the representation – what we are required to believe is the mathematical component. It isn't at all clear, though, what grounds this asymmetry for Pincock. He takes it that it is possible to understand a mathematical representation without believing what it says about the non-mathematical world (this follows merely from it being possible for a mathematical representation to be inaccurate and from it being possible to

[193] Contessa comes close to this when he identifies the representational content of an epistemic representation as "the set of propositions about its target, *t*, that it is valid to infer from its vehicle, *v*" (Contessa ms: 13).

understand an inaccurate mathematical representation). Why, then, can we not understand a mathematical representation whilst similarly not believing in the literal truth of its non-mathematical content?

6.8.2.2. The understanding thesis and the export challenge, again

Whilst noting accurately that Pincock offers no direct argument for the understanding thesis, Walsh, Knox & Caulton suggest that it might get "indirect" support from the export challenge (Walsh, Knox & Caulton 2014: 468). Recall that this can be understood as the challenge to the nominalist to characterise what our theories say about the non-mathematical world (or, relatedly, to tell us what we ought to believe, if not the literal truth of our theories) (see §1.5 for details and some different ways of understanding and motivating this challenge). It is possible to appeal to Kvanvig's distinctions between kinds of understanding to make this suggestion precise.

On the most charitable version of Pincock's understanding thesis, I suggest, the object of understanding is not (contra the letter of Pincock's suggestion) the representation itself. Rather, the understanding thesis should be recast as the claim that in order to understand *what it is that is being expressed about the target system* by the representation, a user of the representation must believe that the mathematical content of the representation is true. Understood along Kvanvig's distinction, the kind of understanding involved is straightforward understanding-that, or propositional understanding. The object of the understanding is not *the representation* but rather is either the proposition 'that *x* is (are) the fact (facts) about the target system expressed by this mathematical representation' or this very collection of propositions about the target system. Even though, on this line, the kind of understanding involved is propositional understanding, it is not problematic that some representations will be inaccurate (or only partially faithful, or completely unfaithful): one can understand *that* a subway map with swapped labels licenses inference to the (purported) fact that Hillhead is one stop from Govanhill without having to understand *that* Hillhead is one stop from Govanhill.[194]

[194] It is of course, on the standard line, *impossible* to understand *that* Hillhead is one stop from Govanhill, because propositional understanding is factive and Hillhead is not one stop from Govanhill on either circle of the Glasgow subway.

Appropriately, given the mention of the various circles of the Glasgow subway, the discussion has circled back round to the export challenge, discussed in §1.5. The response to the challenge articulated there also, therefore, counts as a response to Pincock's understanding argument.

What is it, exactly, that cannot be understood if the mathematics is not believed? It's worthwhile running through the options again. In §1.5 I argued that the demand cannot be to take every claim expressed mathematically and express it without using mathematics: such a demand is to ask Pincock's opponent to demonstrate the falsity of their view, (part of which is the claim) that there are facts about the world that would be ineffable if not for mathematics. On the construal of Pincock's claim as involving propositional understanding, then it doesn't make much sense for the demand to be told what attitude to have towards our scientific theories: but, even if it is, there is a ready response (that our theories ought to be held to be nominalistically adequate). Per the details of the mapping account, and of the account of distinctively mathematical epistemic representation, for each case of representation the user of the representation knows what the target system is, and they also know what the relevant objects and properties in the target system are: this is required in order to set up denotational relations between the vehicle and the target system, and between objects and properties in the vehicle and objects and properties in the target.

One final possibility is that Pincock thinks that the understanding thesis is true because of his idiosyncratic way of articulating (what I have called) the common core of distinctively mathematical scientific representation. Recall that Pincock holds that it is part of the content of a mathematical representation that the (structure of) the vehicle and the (structure of) the target stand in this or that structural relation. On the reformulating here, this isn't quite the role of the structural relation. On the account of distinctively mathematical epistemic representation endorsed above, the specified structural relation is not *part of* the representation. The representation, strictly speaking, consists of the specification of the vehicle and target and the establishing of the two levels of denotation relations. But, if the understanding thesis somehow falls out of this way of construing mathematical representation, then so much the worse for the understanding argument: I have suggested here that understanding the content of the representation to just be the establishing of the two levels of denotation relations and letting the structural relation be involved in grounding the faithfulness, or accuracy, of the representation loses nothing compared to Pincock's presentation. Furthermore, this understanding of the role played by structural relations is fully consistent with what Pincock describes as the *role* of the 'content' of the representation – that it determines the accuracy, or faithfulness, of a representation.

6.9. Conclusion

The conclusions of this chapter concern three main areas. The first is the relationship between theories of scientific representation *simpliciter* and theories of mathematical scientific representation. One live option was that mathematical representations function differently from other epistemic representations like maps: that is, exceptionalism about mathematical representation. Another live option was that recently articulated accounts of mathematical representation (the mapping account and inferential conception) fall into the trap of *identifying* the representation relation with a relation of structural similarity. I argued for the anti-exceptionalist result that mathematical representation can be understood as a special kind of epistemic representation. A mathematical vehicle represents, *qua* denotation, its target in the same basic way that all representations do and the same sorts of facts are responsible for a map's status as an epistemic representation and a mathematical vehicle's status as an epistemic representation. This involved arguing, contra Pero and Suárez, that recent accounts of mathematical representation can be understood in a way such that they do not assign a problematic role to structural similarity. The core account suggested here inherits this property – via being understood using Contessa's account of epistemic representation - and is therefore not vulnerable to the Suárez-Frigg objections. It is no part of the view that structural similarity is *sufficient* for representation. One might press that, given that a great deal of scientific representations are mathematical, it ought to be no surprise that accounts of mathematical representation can be accommodated by an existing account of scientific representation. This point is well taken. Consider first, however, that as the response to Pero & Suárez demonstrates, one can hold that mathematical representation will fit into an existing account of scientific representation whilst being *mistaken* about how existing accounts of mathematical representation relate to such existing accounts. Pero & Suárez took the mapping account to be of the same kind as naïve structural similarity accounts: but this, on closer inspection, is false. Consider second that Contessa's account is an account of epistemic representation *simpliciter* rather than of scientific epistemic representation. Given the assumption that mathematical representations and maps are both the kind of representation that as well as denoting their targets also allow us to learn about them, it is significant that the incorporation here allows these two forms of representation to be accommodated by the same general approach to representation, despite their surface level differences. Consider finally, as demonstrated above when briefly considering the permutation invariance case, thinking about some scientific mathematical examples can feedback into the more general account and suggest modifications. I will return to this in the thesis conclusion.

The second area concerns the relationship between the two prominent accounts of mathematical representation. I argued that, just as functional and informational accounts of scientific representation end up being complimentary once we are careful to distinguish the means from the ends, Bueno & Colyvan's inferential conception and Pincock's mapping account perhaps approach the same core account from different perspectives. At the very least, there is agreement about what relations between vehicle and target make it such that a mathematical vehicle denotes its non-mathematical target, is an epistemic representation of the target and is a (partially) faithful epistemic representation.

The third area concerns the conclusions about the metaphysics of mathematics that can be drawn from this core account. Pincock is one of the few commentators, if not the only to do so *explicitly,* to make an explicit argument that our representational use of mathematics commits us to a kind of realist view. However, I suggested some ways to make Pincock's understanding thesis (the claim that one must have some mathematical beliefs in order to understand a mathematical representation), more precise and argued that none of the ways of doing so make it look like a plausible claim. The implausibility of Pincock's understanding thesis (or, perhaps, its dependence on the plausibility of the export challenge) might suggest a rosy picture for the nominalist: the only direct argument from the representational capacity of mathematics to some kind of realism seems to fail. However, drawing out some of the details of how we represent using mathematics demonstrates that there is still work for a nominalist to do. After all, on the picture set out here, aspects of our representational practices make reference to mathematical structures: structures stand in denotation relations and structural relationships and it is in virtue of these relations that mathematical representation *qua* denotation and epistemic mathematical representation can be achieved. Bueno & Colyvan's argument that the account should be understood as metaphysically lightweight recognises this fact, but reveals itself turn on contentious unresolved issues about the kinds of structural relations involved and distinctions between different kinds of commitment.

Conclusion

In the first two chapters of this thesis, I navigated the respective debates about the various forms of the indispensability argument. The purpose of doing so was to demonstrate that a fruitful way to move towards a resolution of questions of mathematical ontology was to develop accounts of two of our world-oriented uses of mathematics: explanation and representation. The aim of this thesis is not to argue for a particular position in the metaphysics of mathematics, but rather narrow the focus of the debate. A secondary aim is also to say some insightful things about mathematical explanation and representation that are independent of these ontological questions. In this conclusion, I will turn to these two aims for the final time.

7.1. Explanation, representation and ontology

One of the aims of developing the accounts of mathematical explanation and representation was to narrow down, and make more precise, the issues underpinning the realist's contention that mathematics plays the kind of role that is ontologically committing and the representationalist nominalist's contention that it does not. It is worth returning to the two conditionals set out in chapter 1.

7.1.1. Explanation

Consider, first, Explanationism and the Explanation Conditional. According to Explanationism, mathematics sometimes plays an explanatory role in our scientific theories. According to the Explanation Conditional, if mathematics plays an explanatory role in science, then our world-oriented uses of mathematics justify (some form of) mathematical realism. I argued in favour of accommodating these special cases of explanation into Woodward's account of explanation. An outcome of this anti-exceptionalist accommodation is to narrow the debate about explanation and mathematical ontology quite substantially.

Take, first, Explanationism. The account defended in chapter 4 makes clear that mathematics *does* sometimes play an explanatory role. One common way of explaining in science involves offering counterfactual information about the explanandum, and in chapter 4 I suggested that in some cases a mathematical fact does exactly that. How about, then, the Explanation

Conditional? This is a much more fraught question. Still, the question has been narrowed significantly. Rather than turning on judgements about explanation, ontology and truth in general, the question should now be: what must mathematical objects be like in order to play the role of allowing a user access to information about counterfactual dependence? There is, thankfully, a distinction that has been drawn in the recent literature that can aid this question. In a recent discussion, Marcus distinguishes between the playing of a metaphysically explanatory role and an epistemically explanatory role (Marcus 2014: 347). Saatsi introduces what I take to be the same distinction, between "thick" and "thin" explanatory roles: for an object (or fact) to be thickly, or metaphysically, explanatory is for it to bear an "ontic relation of explanatory relevance to the explanandum in question" and for an object (or fact) to be thinly, or epistemically, explanatory is for it to "allow us to grasp, or (re)present, whatever plays a 'thick' explanatory role" (Saatsi 2016b: 1065).[195] So, now, the question is: on the modal account of extra-mathematical explanation, does the mathematics play the kind of metaphysically, thick, ontologically-committing role? It seems as though there are two directions in this might pull, one towards realism and one towards nominalism. I'll sketch both of these out.

One option for the realist, if they agree with the claim in this thesis that the modal account of extra-mathematical explanation is preferable to its rivals, is to exploit its anti-exceptionalist character to provide an improved version of the claim about inferential conservativeness familiar from chapter 2. Recall that the discussion there ended with the suggestion that, even though the straightforward version of the argument from inferential conservativeness hinted at by Baker and Colyvan (see §2.2 and §2.3) fails, the realist might nevertheless quite fairly suggest that the extra-mathematical cases at least *look like* the sort of local cases that their opponent is willing to accept as perhaps involving an ontologically-relevant kind of explanation. The realist might therefore argue that the anti-exceptionalist modal account vindicates the suggestion (discussed in §2.6) that the extra-mathematical cases have lots in common with the local cases after all. This updated version of the inferential conservativeness claim amounts to the twin claims (i) the nominalist is willing to countenance

[195] There are interesting questions about how this distinction maps onto Salmon's distinction between epistemic, modal and ontic conceptions of explanation. Saatsi takes the thick/thin distinction to be a distinction drawn *within* the ontic conception (although given that he thinks that Salmon's modal and epistemic kinds of explanation are unlikely to be ontologically committing, perhaps the thick/thin distinction can be applied in general and it maintained that all sorts of modal and epistemic explanation are thin). Finally, Yablo also introduces a similar sort of carving between three "grades of mathematical involvement" in an explanation, only one of which is even plausibly mathematically committing (Yablo 2012: 1020).

IBE in cases amenable to Woodward-style accommodation (assuming that many of the realist-accepted local cases will be such cases) and (ii) the extra-mathematical cases are also amenable to Woodward-style accommodation. On this line, the modal account is appealed to in order to provide evidence that the extra-mathematical cases *are* in fact a lot like ordinary cases of explanation and to therefore offer a version of the inferential conservativeness claim that doesn't turn on very general, sketchy, abstract remarks about the nominalist opponent already being committed to thinking that *whenever* an object is appealed to in one of our best explanations ontological commitment is licensed to that object.

Nevertheless, there is a convincing nominalist response, one that *also* draws on the details of the modal account offered here. The nominalist could rebut this line of thinking and point to one of the *differences* between the Woodward-style account of paradigmatic causal explanation and the modal account of extra-mathematical explanation offered here. Recall that in §4.6 I stressed that although the invariant generalization that plays a role in paradigmatic causal cases and the mathematical fact in extra-mathematical cases both do work by providing information about counterfactual dependence, they do so in different ways. The invariant generalization *directly* describes the world's patterns of dependence whilst the mathematical fact does so more indirectly, structurally characterising the world in such a way that the world's patterns of dependence are salient to us. A nominalist willing to appeal to the modal account offered here, therefore, could appeal to this difference in order to drive a wedge between paradigmatic causal cases and the extra-mathematical cases, and argue that it is the physical dependence relations that are (in some sense) doing some more fundamental explanatory work (*cf.* Saatsi 2016b: 1062). This is an attractive line of thought, but it is important to stress that its plausibility seems to turn crucially on the counterpossibles question discussed in §4.4 and Pincock's appeal to a *sui generis* kind of dependence, abstract dependence, discussed in sections §3.5 and §4.5. After all, if the nominalist seeks to tie ontological commitment to the dependence relations that the explanandum stands in to the aspects of the world described in the explanans, arguing that the mathematics is used only in order to help us grasp these relations (again, *cf.* Saatsi 2016b: 1062), then a possible route to realism emerges if the case can be made that in order for the modal account to be plausible we must countenance the explanandum standing in dependence relations with an abstract entity of undetermined sorts (as per Pincock's account) or explicitly with mathematical objects (as seems to be suggested by Baron, Ripley and Colyvan's account of extra-mathematical explanation). I think, then, that it should be concluded that appealing to the modal account of extra-mathematical explanation significantly narrows down the questions relevant to assessing the Explanation Conditional: much ends up turning on the role (if any) played by dependence of the explanandum on mathematical facts/objects, rather than on other parts of the physical system. Nevertheless, if this distinction *can* be drawn between how the mathematical fact and the invariant generalization do their explanatory work, it seems as though the ball is in the realist's court. The nominalist can use the resources of the account in chapter 4 to drive a wedge between mathematical and non-mathematical explanation, *even though* both kinds of explanation can be understood using a Woodward-style account and *even though* it is left open whether any of the extra-mathematical explanations are causal explanations. The realist ought to provide an account of why mathematical objects must exist in order for the mathematical fact to play the kind of explanatory role that it does.

7.1.2. Representation

Consider, second, Representationalism. This stated that mathematics *merely* plays a representational role. There is reason to think that this is a somewhat delicate question, turning on exactly what one draws the line on mathematics playing an explanatory role. One can imagine someone hard-headedly maintaining that unless mathematics plays a thick, or metaphysical, explanatory role then it is only representational. I am inclined to think that this is a terminological question and think that it ought to be granted that mathematics *does* play an explanatory role and, therefore, Representationalism (as stated in the introduction) is false. What, then, of the Representation Conditional? Even though it is, I have suggested, *false* that mathematics only plays a representational role, there is still reason to be interested in the ontological commitments of mathematics' representational capacity. If the nominalist can make the case, as suggested above, that mathematics' explanatory role does not straightforwardly license ontological commitment, the realist might still press that its *representational* capacity is committing (either to truth-value realism, as Pincock claims, or to a stronger form of realism).

The central question, here, is what mathematical objects have to be like in order to play a role in representing. Recall that there are three relations involved in a case of mathematical representation. The two levels of denotation relation seem straightforward for the nominalist to accommodate: but what about the holding of a structural relation between the target system (suitably carved up into objects, properties and relations) and the mathematical structure? There is, of course, the natural nominalist suggestion (discussed in chapter 1) that mathematics is being used here to express a claim about the target system (that it is structured in such a way) and that neither mathematical existence nor mathematical truth is required in order for this to take place (that expressing about the target system that it *would* stand in such and such a structural relation to a given mathematical structure *if* the mathematical structure existed does not require that the mathematical structure *in fact* exists). A full defense of this fictionalist line of thinking is beyond the current scope, which has been to shed light on this representational use of mathematics. However, I have considered two (truth-) realist lines of response that threaten this nominalist accommodation of mathematics' representational function: the export challenge (§1.5 and §6.8) and Pincock's understanding thesis (§6.8). It is worthwhile considering a second possible line of realist response to the account of mathematical representation endorsed here. One might press that, even if mathematics' representational capacity does not generate commitment to the mathematical structures that seem to be involved (because they are only used to express information about the target system), there is nevertheless a commitment to the structures that the *target system* is taken to instantiate (those that are taken to stand in structural relations with the mathematics). However, this move seems to amount to moving the debate away from distinctly mathematical considerations and towards more general questions about the metaphysics of properties and universals: questions that must be addressed by anyone willing to take the world to be structured. Whether or not one can interpret talk of the world's structure as just a way of talking about the objects, properties and relations that make up the world (as opposed to having to accept the existence of a structure, instantiated by the world, that exists over and above these components of the world) is a far more general, not distinctively mathematical, metaphysical question.

7.2. Exceptionalism and anti-exceptionalism

One of the secondary aims of this thesis was to make the case for anti-exceptionalism about mathematical representation and explanation. The best way to argue for anti-exceptionalism about some phenomena (be it the methodology of philosophy (Williamson 2007; Williamson 2013a), theory choice in logic (Hjortland 2017; Maddy 2002) or mathematical explanation (chapter 4 of this thesis)) is to do the work and demonstrate that the phenomena can be accommodated by some existing, more general, account. In chapter 4, I discussed the extension of the modal account of explanation to extra-mathematical explanation and argued that it is superior to rival accounts. In chapter 6, I argued that the common core of recent accounts of mathematical representation can fruitfully be accommodated by an existing account of representation: Contessa's epistemic framework. These two arguments acted as a corrective to those who take extra-mathematical explanation to be *sui generis* and also to those who take recent accounts of mathematical representation to belong to a tradition that takes structural similarity to be *sufficient* for representation.

However, anti-exceptionalist accommodations of phenomena can threaten to flatten and obfuscate features that are genuinely idiosyncratic. For example, the logical anti-exceptionalist's claim that simplicity is a norm that governs theory choice in both logic and science might hide the fact that the notions of simplicity in play in these two disciplines are, in fact, radically different and that they may differ in the extent to which they are truth-conducive. In this case, a claim that is true at a sufficient level of abstraction and generality in fact *hides* important differences that become apparent once we attend to the phenomena at a more fine-grained level.

The anti-exceptionalist accounts offered in this thesis do not flatten their respective phenomena in this way. Rather, the anti-exceptionalist accounts offered here respect what makes mathematical explanation and mathematical representation interestingly different from other kinds of explanation and representation, *even though* they end up sharing many of their core features with representation and explanation simpliciter. One crucial aspect of the anti-exceptionalist account of explanation offered here is that even though the mathematical fact and the invariant generalizations play the same essential role they do so in different ways. Invariant generalizations directly describe a small part of the world's patterns of dependence – even if it is described *using* mathematics. Mathematical facts played this core explanatory role in a slightly different way: a mathematical fact - expressing some claim about how things are mathematically, *not* directly describing the world's systems of dependence – can nevertheless play the core explanatory role in virtue of the fact that the target system (suitably interpreted) can be understood as being an approximate instantiation of the axioms governing the mathematical system that is the subject matter of the mathematical fact. Rather than being a surface level difference, the unique way in which mathematics plays this role in fact generates interesting future questions and, as demonstrated above, can also play an important role in adjudicating metaphysical debates.

Furthermore, the discussion of some distinctively mathematical epistemic representations led to fruitful potential revisions of the general account under which they are subsumed. As seen when discussing the inferential conception, an idiosyncratic feature of mathematical representations may be that, in some cases, the user of the representation taking the target system to stand in a structural relation to the vehicle is what sets up the denotation relations – and that this very same structural relation plays a second role of grounding the representation's faithfulness. Second, as will become apparent below when discussing the potential limits of the epistemic representation framework, the mathematical cases may require a bifurcation of the notion of an interpretation.

7.3. The limits of anti-exceptionalism and the prospects of monism

7.3.1. The limits of the epistemic representation framework

The account offered of epistemic representation was originally intended to be fully general. If this is the case, then in any case of (partially) faithful epistemic representation, there will be a denotation relation between the vehicle and (purported) target, a set of denotation relations between parts of the vehicle and parts of the target (which jointly constitute an interpretation) and there will be structural similarity between the vehicle and target grounding the representation's faithfulness. This is equally true for non-scientific epistemic representations, non-mathematical epistemic representations and mathematical epistemic representations. It, nevertheless, leaves open questions about the nature of denotation and the exact mechanism via which such representations might activate mental representations: it does not address Callender and Cohen's question about the relationship between fundamental and non-fundamental representations (see §5.2).

There is a more pressing open question, however. Recall that in cases of permutation invariance, it seemed less-than-straightforward to understand *both* levels of denotation relations obtaining: whilst the top-level denotation relation between the vehicle and target system may well obtain, facts about the non-individuality of the objects that make up the target system make reconstructing the representation as involving denotation relations between the vehicle objects and target objects look difficult. There are two options, as I see it. In his initial characterisations of epistemic representation, Contessa notes that (what he calls) analytic interpretations are only one kind of interpretation, and that other kinds are conceivable (Contessa 2007: 58). So, the first route to accommodating these permutation invariance cases (and any other cases where object-level denotation relations are hard to locate) involves widening the notion of an interpretation such that it can be the case that the vehicle has been interpreted in terms of the target (and so can license inferences) without it being the case that object-level denotation relations have been established. This, for example, might be a kind of interpretation that only establishes denotation relations between properties and relations, but *not* between objects of the vehicle and target. The viability of this option, of course, turns on the details. If it fails, the second option involves giving up the interpretational account of epistemic representation as a fully general account: admitting that it breaks down when, for example, it is not straightforward to understand the target system as consisting of intrinsically individual objects. Although this question about the bounds of the epistemic representation account is left unanswered here, it is nevertheless an important lesson that some distinctively mathematical representations (like the symmetry representations) raise potential challenges for accounts of representation given at the level of generality of Contessa's. In an important sense, the 'direction-of-travel' between accounts of scientific representation *simpliciter* and accounts of mathematical representation goes both ways: the epistemic framework sheds light on mathematical representation but mathematical representation *also* signals when the conditions of the epistemic framework are too strong.

7.3.2. Explanation

Extra-mathematical explanations are often taken to challenge the view that all explanations are causal – so it'll be helpful to connect up the discussion in this thesis to this debate. In a recent survey paper, Reutlinger delineates three views about non-causal explanation (Reutlinger 2017a). According to causal reductionism, *all* explanations are causal explanations. According to monism, there is a general account that can accommodate *both* causal and non-causal explanation. Finally, the pluralist agrees with the monist that there are non-causal explanations but claims that there is not a fully general account and, accordingly, at least two accounts of explanation must be given.[196]

[196] Reutlinger discusses reductionism, monism and pluralism as views about *scientific* explanation. There are reasons for thinking that these views are most interesting understood as views about explanation *simpliciter*. Many questions about explanation cut across the scientific/non-scientific divide (if there is a substantive divide at all). Theories of causal explanation, for example, are not offered as theories of *scientific* causal explanation but of causal explanation *simpliciter*: it is a mark against a theory of causal explanation if it cannot accomodate Billy breaking a window with a stone. Similarly, whether or not one must offer a unified account of intra-mathematical explanation and extra-mathematical explanation is an interesting debate, but one that cuts across the scientific/non-scientific divide.

How does anti-exceptionalism about extra-mathematical explanation relate to these more general views? Recall that at the end of chapter 4, I suggested that whether or not the accommodation of the extra-mathematical cases into a Woodward-style account revealed that the explanations were in fact causal (contra the received wisdom) turned on delicate questions about the interventions involved. Indeed, it is possible that *some* extra-mathematical explanations are causal whilst some are not. If all the interventions involved in extra-mathematical explanations should be understood as causal interventions, then it seems as though the account here is a piece of evidence in favour of reductionism, the most conservative of the three views. If, however, not all of the interventions can be given a causal reading, then it seems as though the account offered here is a piece of evidence that tells *against* reductionism and *for* monism. And, of course, the truth of the view here is consistent also with pluralism: pluralism will end up being true if there is *another* kind of explanation that does not yield to either a reductionist or a monist understanding. The case for monism ought to be built bit by bit. This raises another interesting question for future work: how far the modal account might go. In previous chapters I flagged up recent work that extends a Woodward-style account to different kinds of non-causal explanation (Bokulich 2011; Reutlinger 2014; Rice 2013; Saatsi & Pexton 2013), work that this thesis builds on – below I suggest two pieces of new ground.

7.3.1 Intra-mathematical explanation

I do not think there are any positive reasons to *require* our account of extra-mathematical explanation to also cover intra-mathematical explanation (as argued in §4.1). Nevertheless, the success of a Woodward-style account in shedding light on extra-mathematical explanation suggests that extending theories of causal explanation to the intra-mathematical case might not be so much of a dead-end as is often held. Indeed, some of what Steiner says in setting out his 'characterizing properties' account of intra-mathematical explanation has an interventionist ring to it:

> It must be evident, that is, that if we substitute in the proof a different object of the same domain, the theorem collapses; more, we should be able to see as we vary the object how the theorem changes in response. (Steiner 1978: 143)[197]

[197] Thanks to Josephine Salverda for drawing my attention to this aspect of Steiner's account.

A *prima facie* worry about extending Woodward's account in this way (or, alternately, in modifying Steiner's account in this way) is that unlike the extension of the modal account to mathematical explanations of non-mathematical facts, a modal account of mathematical explanations of mathematical facts will, it seems, *necessarily* appeal to counterpossibles. However, the proponent of a fully general modal account, though, would be able to draw on the work of Baron, Colyvan and Ripley discussed in chapter 4 in order to provide a basis for thinking about how manipulations of mathematical facts ramify to other facts (Baron, Colyvan & Ripley 2017). Even though, as I argued, such work isn't required in order to extend the account into the extra-mathematical case, it will be invaluable in future attempts to extend it into the intra-mathematical case. This brief discussion is a long way from an *argument* that a unified account can be had of extra-mathematical and intra-mathematical explanation: however, this is a potentially fruitful avenue of future research.

7.3.4. Social structural explanation

Here's another potential way to extend the account. Consider, social structural explanations: explanations that appeal to facts about social structures to explain phenomena. These explanations, only recently discussed in any detail, are ripe with interesting philosophical features: they sit on the line between scientific explanations and (something like) *political* explanations and, similarly, are offered up in both descriptive and normative contexts. Recent work on such explanations suggests that they may belong to a wider class of structural explanations (Haslanger 2015) but does not attempt to shed light on it by appealing to existing accounts of explanation. This, then, seems a natural novel extension of the approach to mathematical explanation taken in this thesis, given that mathematical explanations are naturally understood as structural explanations of a different kind. Clearly, it will not be trivial to apply the account. Nevertheless, a modal account along the lines of this thesis seems like it is well suited to cashing out the claim that some social phenomena can depend not only on micro-facts about the agents involved but also (or instead) on wider, structural, abstract features of the social world in which they live, much as characterising the structural features of a bridge system can tell us what its non-tourability depends on.

7.4. The world, structure, and the structure of the world

In both the discussion of mathematical explanation and mathematical representation, the idea of the world having a structure played an important role. That the world is structured is what allows a structural relation between target and vehicle to ground a representation's faithfulness: without the assumption that the world is structured, the suggestion that a mathematical vehicle and the world stand in a structural relationship seems like a category mistake. Remember, also, that the world being structured such that it instantiates a collection of axioms is what allows the mathematical fact to have explanatory power. It is common to find talk of *the* structure of the target system, or *the* structure of the physical domain. However, there are multiple ways of imposing structure onto the world and onto the target systems present in representations: the world can be carved up into objects, properties and relations in very many ways. For reasons familiar from the discussion in chapter 6, it is not at all straightforward to identify one such imposable structure as the world's *actual* structure – even putting aside the myriad problems with natural kind talk, limiting the structures to those that carve up the system along natural kind terms does not yield one privileged structure. Nevertheless, as I stressed in chapter 6, there is little reason to think that proponents of accounts of mathematical representation that appeal to structural relations, nor those working in the partial structures tradition, are naïve regarding this point: Bueno & Colyvan explicitly raise this worry and Pincock, too, takes the specification step to involve selecting one particular way of carving up the target system into objects, properties and relations, implying acknowledgement that the world does not come readily-carved, as it were (see §6.2).

So, this issue has not gone unnoticed: but is it harmless? Perhaps so, as long as it is kept in mind that the inferences generated are also to be relativized to the chosen structure. One remaining worry, though, was the role that the world being structured plays in grounding the faithfulness of a given representation. Recall that, for example, the faithfulness of the Glasgow subway map was explained by the fact that the map (the vehicle) and the subway system (the target) are structurally similar. Yet, as is now familiar, talking of the map and the system being structurally similar is only meaningful once one possible way of imposing structure on the system is privileged. A possible outcome is that all inferences generated by epistemic representations are relative to a chosen imposition of structure on the target system. *Prima facie,* this is not yet an objection to the account rather than a feature of it. The relevant structure that the target system is taken to instantiate is relevant because it has been taken to be relevant by the user of the representation: there is no intractable question about what makes it such that *this* structure of the target system is that which is appealed to in *this* representation. There will be those who are unhappy, of course, with this state of affairs: those who will still demand a way to privilege some of the structures that the target system could be taken to instantiate that does not appeal to the interests and choices of the user of the representation. This has become, then, an instance of a more general and fundamental worry. Someone who seeks an answer to this question can be directed to the literature on Newman's objection: especially those responses that try to carve out some privileged structures (Psillos 1999; Votsis 2004), although see Ainsworth 2009 for critical discussion of these and other strategies.

7.5. Some lessons

Here, then, are the lessons of this thesis. The first is that, if one is invested in the debates about mathematical ontology that take as their lead our world-oriented uses of mathematics (as opposed to so-called 'easy-arguments'), it is crucial to turn first to descriptive questions about how mathematical explanation and representation function. It is only with these accounts in hand that it is possible to discern whether claims like the Explanation Conditional and Representation Conditional are true. I have also suggested that this project ought to be carried out by attending to the ontological ramifications of the two accounts endorsed here: the modal account of extra-mathematical representation and the account of distinctively mathematical epistemic representation. The second lesson is that reasons given for not extending accounts of causal explanation to purportedly non-causal explanations often reveal themselves to be unconvincing on closer inspection: and that whether or not a kind of explanation is causal or not may not turn on the properties of the objects appealed to in the explanation but on the nature of the interventions involved in the explanation. Both of these lessons have consequences for the emerging debates about the existence of non-causal explanation and the prospects of a fully general account of explanation.

Appendix A: Inference rules Contessa's interpretational account of epistemic representation

In this appendix I set out Contessa's inference rules.

In addition to interpreting the target in terms of the vehicle (which amounts to establishing denotation relations between parts of the former and parts of the latter), epistemic representation also requires inference rules. Contessa (Contessa 2007; Contessa 2011; Contessa ms) sets out some inference rules, making explicit that different sorts of interpretations may come with their own set of inference rules. He offers the following rules for analytic interpretation.

> **Rule 1:** If, according to the analytic interpretation of *v* in terms of *t*, o^V_i denotes o^T_i it is valid for the user to infer that o^T_i is in the target if and only if o^V_i is in the vehicle.
>
> **Rule 2:** If, according to the analytic interpretation of *v* in terms of *t*, o^v_1 denotes o^T_1, …, o^V_n, and ${}^nR^V_k$ denotes ${}^nR^V_k$ it is valid for the user to infer that the relation ${}^nR^T_k$ holds among o^T_1, …, o^V_n if and only if ${}^nR^V_k$ holds among o^V_1, …, o^V_n.
>
> **Rule 3:** If, according to the analytic interpretation adopted by the user, o^v_1 denotes o^T_1, o^v_1 denotes o^T_1, …, o^V_n and ${}^nF^V_k$ denotes ${}^nF^T_k$, it is valid for the user to infer that the value of the function ${}^nF^T_K$ for the arguments o^T_1, …, o^T_n is o^T_i if and only if the value of the function ${}^nF_k{}^V$ is o^V_i for the arguments o^V_1,…,O^V_n. (Contessa 2007: 61).

Appendix B: The partial structures framework

In this appendix I set out the partial structures framework, which is appealed to in Bueno & Colyvan 2011. More detail about the framework is given in work by its proponents (da Costa and French 2003; Bueno, French and Ladyman 2002; French 2003; French 2014).

Partial structures

Let relations be understood by specifying the *n*-tuples for which they hold. The partial structures approach introduces a third class of *n*-tuples: in addition to the classes for which the relation holds and for which they do not, a third class is introduced that is made up by the *n*-tuples for which it is not defined whether or not the relation holds.

A partial structure is an ordered pair $< \mathcal{D}, R_i >_{i \in I}$, where $\mathcal{D}$ is a non-empty set, $R_i, i \in I$ is a family of partial relations (where I is an index set).

A partial relation $R_i, i \in I$ over $\mathcal{D}$ is a relation which need not be defined for all *n*-tuples made from elements of $\mathcal{D}$. Each partial relation R can be understood as an ordered triple $< R_1, R_2, R_3 >$. R_1, R_2 and R_3 are mutually disjoint sets, with $R_1 \cup R_2 \cup R_3 = \mathcal{D}^n$, such that R_1 is the set of *n*-tuples that belong to R, R_2 is the set of *n*-tuples that do not belong to R and R_3 is the set of *n*-tuples for which it is not defined whether or not they belong to R.

Partial isomorphism and homomorphism

The partial structure framework allows for the definition of partial isomorphism and partial homomorphism in a way that is analogous to the standard notions of isomorphism and homomorphism. The below definitions are taken from Bueno & Colyvan 2011.

Let $S = < \mathcal{D}, R_i >_{i \in I}$ and $S' = < \mathcal{D}', R'_i >_{i \in I}$ be partial structures, where R_i and R'_i are (for simplicity) binary partial relations. A *partial* function *f*: $D \rightarrow D'$ is a *partial isomorphism* between S and S' if (i) *f* is bijective and (ii) for every *x* and *y* $\in D$, $R_1xy \leftrightarrow R'_1f(x)f(y)$ and $R_2xy \leftrightarrow R'_2f(x)f(y)$.

Let $S = <\mathcal{D}, R_i>_{i\in I}$ and $S' = <\mathcal{D}', R'_i>_{i\in I}$ be partial structures, where R_i and R'_i are (for simplicity) binary partial relations. A *partial* function *f*: $D \rightarrow D'$ is a *partial homomorphism* from S to S' if for every *x* and every *y* in $\mathcal{D}$, $R_1xy \rightarrow R'_1f(x)f(y)$ and $R_2xy \rightarrow R'_2f(x)f(y)$.

Partial truth

See da Costa & French 2003 for definitions of the notion of partial truth using the partial structures framework.

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